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Astronomical Summaries

As a part of the commemoration of the completion of the first fifty volumes of this magazine, we, following a suggestion by one of the collaborators, have arranged for a series of papers on various aspects of astronomical development in the half-century which has elapsed since the beginning of POPULAR ASTRONOMY.

These papers are intended to give, without too many details or too many technicalities, a résumé of the progress made and possibly an indication of the probable direction of future investigations —a taking of stock and a look forward.

Unavoidable delays have prevented the inauguration of this series until late in Volume 51. We are pleased to present here the following paper, introductory to the series, prepared by one whose thorough comprehension of astronomical literature is unquestioned and whose contributions to it, at least in the latter part of the period under consideration, are well known.

Other papers will appear in succeeding issues. EDITOR.

Fifty Years of Progress in Astronomy

By OTTO STRUVE

"The main object of astronomy, as of all science, is not the collection of facts, but the development, on the basis of collected facts, of satisfactory theories regarding the nature, mutual relations, and probable history and evolution of the objects of study." This quotation is the introductory sentence of a very stimulating article[1] by Professor H. N. Russell, which appeared under the title "Some Problems of Sidereal Astronomy" in 1919, under the auspices of the National Research Council. After the first world war had ended, there occurred in the United States and elsewhere an unprecedented and almost uncontrollable upsurge of interest in all the sciences. In order to direct this interest into useful channels, the National Research Council undertook to publish several surveys on important problems in the different sciences. As Professor Russell pointed out, there are two policies of investigation open to astronomers: 1) to collect observational facts without regard to their ultimate usefulness in the solving of the general problems of astronomy, or 2) to direct the observational work along those lines which have a direct bearing upon the solution of general problems. Although it would seem that the choice of the procedure is obvious if the question is put in this way, there has always existed a tendency in astronomy to follow the first, and not the second, procedure.

A physicist would consider it incomprehensible that anyone should find satisfaction in observing a phenomenon only because it is measurable. But, because the intervals of time required to record the progress of an astronomical phenomenon may greatly exceed the lifetime of an astronomer, he, much more than the physicist, is inclined to lose contact between observation and interpretation. When he measures a double star, or when he records the spectral lines of a peculiar star whose past history has shown occasional outbursts perhaps once per century, or when he takes a first-epoch plate for the determination of proper motions, he may remember that all of these tasks have a sense only if they have a bearing upon some important question connected with a knowledge of the stars' masses, or with the unraveling of a star's evolution, or with the understanding of the dynamical properties of the galaxy. But it is very easy to forget these ultimate aims and to be satisfied with the process of observing as such. This drawback is not so serious in theoretical work, but I suspect that even there the temptation may be great, at times, to carry through a numerical integration because the process is almost of a routine character. And there is always the hope that someone else may find the work useful. This attitude cannot be justified and it is very important that astronomers should re-appraise their problems and make careful plans for future work.

Astronomers will welcome the initiative of POPULAR ASTRONOMY to publish a series of articles on a number of current topics in astronomy, and it is to be hoped that these papers will serve as guides for the post-war programs which will be developed at our observatories and universities.

The editor has asked me to prepare, as an introduction to these papers, a short review of the progress which our science has made in the last fifty years. I shall not attempt to give a complete history in this paper, since that will undoubtedly be done by the various authors of this series. But it seems to me to be of interest to make a more general survey and to uncover those characteristic trends in astronomy which have developed during the past era of fifty years—from 1893 to 1943. Perhaps a recognition of these trends will help us to understand the progress our science has made and to prepare for intelligent work in the future.

That progress in astronomy during the past fifty years has been large and impressive, there can be no doubt. But I sometimes wonder whether we are not often exaggerating the advances made in our own lifetimes or in the generation just preceding our own. Perhaps we lack a proper basis for making a comparison. Achievements have been great. But could they not have been much greater if the world had been spared two world wars of unprecedented ferocity, and if political systems between the wars had been more favorable to science, or if astronomers had planned their work more carefully? In the brilliant rays of important advances, must we not also look into the dark recesses of incompetence and failure?

About fifty years ago, to be exact, on September 9, 1892, Edward Emerson Barnard, then a young astronomer on Mount Hamilton, pointed the 36-inch refractor of the Lick Observatory at Jupiter,[2] and "in carefully examining the immediate region of the planet Jupiter . . . detected an exceedingly small star close to the planet and near the third satellite." Barnard at once suspected it to be a new satellite. The wires of the micrometer were broken and before he could repair them "the object disappeared in the glare about Jupiter." On the next night he observed the object again, and a few weeks later[3] he announced its period as $11^h\ 57^m\ 20^s.5$. All observations of the fifth satellite of Jupiter were made visually. Had Barnard not been given the use of the 36-inch telescope, or had he been a less enthusiastic observer, it is doubtful whether this satellite would have been discovered at all. For we must admit that the past fifty years have seen the gradual suppression of visual methods by photography, and it is more than doubtful that a photograph could have been used to discover the fifth satellite. I am not even certain that a photograph of it has ever been taken—an accomplishment which would be in a very different category from making the visual discovery. The decline of visual astronomy is not confined to satellites. In the beginning of our era Burnham, Aitken, Barnard, Hussey, Doolittle, and many others assiduously observed visual double stars, and placed on a firm basis our entire knowledge of the masses of the stars. At the present time only two or three active and keen observers continue this work regularly. Yet, double-star work undoubtedly promises even better rewards now than it did fifty years ago!

At some of our large observatories, lack of skill in visual methods has brought about stagnation in research on planetary surfaces—a field of endeavor which was of exceptional fascination to many of our predecessors. Schiaparelli and Barnard never missed a night of good seeing when Mars or Jupiter was in opposition!

Astronomers have been accused during the past twenty years, or so, of having permitted this lack of skill to make them unresponsive to the repeated claims of Father Hagen and his followers that large regions of the sky, principally in the higher galactic latitudes, are covered with faintly luminous clouds, brown in color and therefore not recorded on the usual long exposures with wide-angle lenses. Among Father Hagen's disciples are astronomers of distinction and unquestioned ability. The brothers Friedrich and Wilhelm Becker have on numerous occasions repeated their belief in the physical reality of these clouds. Yet, it appears from the literature that few astronomers have even attempted to test Father Hagen's observations. It is not enough to say that the clouds are unreal because they violate our ideas of the structure of the galaxy. We must know why good observers have claimed that they saw these objects and devise experiments to show whether or not the observations have a physiological (rather than a physical) explanation.

To remedy the lack of visual observing we must, of course, not diminish effort in other fields. But we must train young men in this type of work and stimulate their interest in it. Above all, we must see to it that they are not made to feel that they are asked to work on "antidiluvial" problems and with "pre-reflectorian" telescopes. We can tell them that all capable visual observers who have made the test have found that the large reflectors are superb for visual work. I recall a remark by a European astronomer who had spent his entire life observing the satellites of the solar system with Europe's largest refractors. After a visit to Mount Wilson for the Solar Union meeting in 1910 he said, a little wistfully: "America's large reflectors are unsurpassed for precise micrometer work on double stars and satellites and the best view of a planet I have ever had was with the sixty-inch reflector at Mount Wilson." This view has been supported by several capable observers who have used a micrometer in connection with a very large reflector and who have had ample opportunity to compare large telescopes of both types at several observatories.

It may be thought that by urging the use of large reflectors for visual work I am trying to contribute to the decline of the refractors. We have here another characteristic of the last fifty years: the refractor is considered an antique; the reflector is in the ascendancy. The Yerkes 40-inch refractor was installed nearly fifty years ago. Larger refractors were attempted only twice: the ill-fated 50-inch objective made many years ago for a Paris exposition was never placed into actual use and was, if I remember correctly, offered for sale some years before the outbreak of the present war. The second was ordered by the Russian government before 1914, in England, and was intended for one of the southern stations of the Pulkovo Observatory, Nikolaieff or Simeis. This instrument has never been finished. There have, of course, been placed in operation a number of very fine refractors—visual and photographic—of more moderate size. The Yale and the Michigan telescopes in Africa, the Berlin-Babelsberg refractor, and several others come to mind.

Lest anyone should think that I am trying to disparage the type of instrument which I myself have been using most extensively during the past twenty-two years, let me say at once that after four years of work with the 82-inch reflector of the McDonald Observatory I am more convinced than ever that the refractor has a permanent place in observational astronomy. Its importance lies predominantly in those fields where it is important to preserve for long periods of time the exact shape and the adjustment of the image-forming device. A large mirror acts like a pancake: if you raise a supporting pad at the bottom too high, it bulges; if you put too much pressure on the edge supports it folds over; if you tilt it, its behavior is different from that observed when you place it horizontally; if the temperature changes it becomes astigmatic;

if the edge supports are too loose it rolls in its cell and causes the optical axis to wander from one place to another. No wonder that parallax observers clamor for a good refractor! Let us be sure, then, that the idea should not gain ground that the refracting telescope is "played out."

The past fifty years have seen the rise of many new and powerful observatories. The Lick Observatory was completed in 1887. Yerkes came ten years later. The Lowell Observatory was founded in 1894 and the Mount Wilson Observatory in 1904. The Sproul Observatory and the Van Vleck Observatory were opened in 1911 and in 1915, and the Steward Observatory of the University of Arizona in 1916. The Victoria 72-inch and the Mount Wilson 100-inch were erected in 1918. The Perkins Observatory started work in 1930 with a 69-inch telescope. The David Dunlap Observatory in Toronto with its 74-inch was founded in 1932. The McDonald 82-inch was dedicated in 1939. The new Schmidt telescope of the Case School in Cleveland started operations in 1942 and the Schmidt reflector at Tonanzintla, in Mexico, was formally opened by President Camacho in February, 1942. At the same time many other powerful instruments were made for some of the older observatories. The U.S. Naval Observatory built a Ritchey-Chrétien reflector; Yale, Michigan, and Harvard organized powerful stations in Africa. Lick Observatory acquired a 20-inch photographic telescope of the Ross type—the largest now in existence. Harvard built two sixty-inch reflectors and erected a coronagraph at an altitude of 11,500 feet. The McCormick Observatory installed a powerful objective-prism instrument for a spectroscopic survey of faint stars. The University of Indiana built a Schwarzschild-type reflector and obtained the use of a private reflector at the Goethe Link Observatory. A most extraordinary development in solar research took place through the construction of the powerful solar telescopes of the McMath-Hulbert Observatory, which is affiliated with the University of Michigan. The latest addition to the growing list of large telescopes is a 60-inch reflector at the National Observatory of the Argentine Republic at Cordoba.

Most of these developments took place in America. In Europe, economic and political difficulties prevented a similiar development. However, Russia installed a 40-inch reflector of English make at Simeis and several smaller, but powerful and modern, instruments of Russian make at Abastumani in Georgia. Germany built a powerful observatory at Babelsberg, with a 48½-inch reflector, and modernized several others. German reparations paid for the modern equipment at Uccle, in Belgium, with which relatively few results have been announced, and for some instruments in Italy. Holland embarked upon extensive plans for an observatory in Africa. Greenwich and Edinburgh installed new reflectors of moderate size, and Saltsjöbaden, near Stockholm, was newly created with modern equipment of the highest quality.

In France a new modern observatory was under construction when the war broke out in 1939, and elaborate plans were under way for Liége, in Belgium. On the whole, it is quite obvious that in observational facilities Europe lagged far behind America and South Africa. The chief cause was undoubtedly the war and the fact that nearly all European observatories are operated by their governments. And governments rarely have enough money for pure research, especially when they are preparing for war.

This brings us to the consideration of a very important point. No one can doubt that private munificence, which was largely responsible for the rise of American observatories in the past, will not be able to function in this manner after this war, and the question arises how the old observatories will maintain and improve their equipment and how new observatories will be created. A temporary expedient may be found in the existing large foundations, but the ultimate solution probably lies in government control of some sort. Such a course will have many disadvantages. In some countries such control has been well-nigh disastrous. On the other hand, we must recognize the extraordinary fact that most astronomers in Russia and many of their instruments were evacuated by the government to safe places before the German invasion enveloped them. Government control would mean protection. It may also mean more intelligent planning than we have had in the past. I venture to suggest that the past fifty years may have seen the last of the privately endowed observatories, and that the next half century will see the gradual assumption by the taxpayer of the responsibility for developing pure research in astronomy.

Careful planning of the organization of new observatories in the hands of a responsible organization should remedy one characteristic defect of the old system—the over-production of telescopes for the northern hemisphere and the neglect of the scientifically more important southern hemisphere. Somewhat tentative efforts to provide a very large reflector for the southern hemisphere were already under way some years ago. The next fifty years will almost certainly bring an increase of effort in the southern hemisphere.

During the past fifty years there has been a marked tendency in most large observatories toward decentralization. In the beginning of the period which we are considering the directors had far greater authority than they have now and their scientific interest largely dominated the observational programs. There has gradually been developed a recognition of the interests of the individual workers and in many observatories all but a few untrained computers are engaged in individual research. This change has brought gains as well as losses. It has often been remarked that such monumental coöperative projects as the *Astronomische Gesellschaft Catalogue* of star positions and the photographic *Carte du Ciel* have done much harm by virtually killing the ambitions and scien-

tific aspirations of hundreds of the younger astronomers in Europe. It is true that participation in a large routine program, developed and supervised by the director, had little in it to attract a brilliant young man intent upon making a name for himself. Doubtless, some of the more independent persons became discouraged and gave up the study of astronomy. Others, less independent, often became mere office holders. I recall a conversation I heard in a German railroad train, following the 1913 meeting of the *Astronomische Gesellschaft*. Dr. X was an assistant in a German observatory and had been engaged in some routine program with a meridian circle. His appointment had expired on December 31, and he was telling with considerable delight how at the exact second of midnight he had interrupted the transit observations of a star and written *finis* in the official record-book. It is certain that this kind of attitude no longer exists among the younger astronomers. But we must also recognize that the older system did give us the *A. G. Catalogue* and many other results of lasting value. It is doubtful, to say the least, that astronomy would have gained if Dr. X and men like him had been given greater freedom in the choice of their problems. For better or for worse, the democratization of our observatories has rendered it less likely that we shall see the execution of new projects having a scope like the *A. G. Catalogue,* or even like W. W. Campbell's enormous radial velocity program at the Lick Observatory, or like the Harvard surveys of stellar spectra which were organized by E. C. Pickering and largely executed by Miss Maury, Miss Cannon, and Mrs. Fleming. It is true, of course, that Harvard is even now continuing the work on spectral classification and that Vyssotsky has embarked upon a program of objective-prism spectroscopy which embraces the entire northern sky and reaches much fainter stars than the *Henry Draper Catalogue*. It is also true that Potsdam and Bergedorf have made large routine surveys of stellar spectra. Yet, I believe that these projects rather support my contention than disprove it. The anonymous character of the earlier projects has given way to individual recognition, and mass programs, participated in by dozens, if not hundreds, of highly trained astronomers, have given way to what are essentially one-man programs, carried out with the greater efficiency of modern methods and aided by a few anonymous computers.

Another important change which has contributed to the disappearance of large routine projects is the decline of international coöperation. In the beginning of our era internationalism in science was very strong. In 1887 the director of the Pulkovo Observatory of Russia presided at an international conference in Paris where the plans for the *Carte du Ciel* were adopted and Admiral Mouchez of France was elected chairman of the permanent commission of the project. Numerous observatories all over the world obtained photographic refractors and started taking plates for the charting of all stars to the 14th magnitude and for cataloguing them to the 11th magnitude. Unquestionably, the *Carte du*

Ciel represents the greatest international project in astronomy of all times. It is not yet finished and from a modern point of view its methods are old fashioned and wasteful. But it stimulated the development of astronomical photography and gave us a source of inestimable value for the study of proper motions. The catalogues themselves, and even the charts, constitute now an auxiliary system of star positions for the determination of the spherical coördinates of comets and asteroids. In the second half of our era, the two world wars, as well as the general political and economic unrest of the world, made it impossible to continue such international projects or to start new ones. It is proper to say, therefore, that the last fifty years represented a decline of internationalism in astronomy. The *Astronomische Gesellschaft* lost its international character in the first world war, though its leaders tried to preserve the appearance by holding every other meeting outside of Germany. The last such outside meeting was held in Danzig a few weeks before the fatal days of August, 1939. The choice of this city seems to characterize the hollowness of the pretense of internationalism. Our era is characterized by the growth of the *International Astronomical Union,* which was never truly international, but which accomplished a measure of solidarity between astronomers of many, if not of all, countries. In recognition of the universal trend away from internationalism the *Astrophysical Journal* in January, 1942, dropped its international board of editors established by G. E. Hale when the *Journal* was created in 1895. Although other, more recent journals, like the *Zeitschrift für Astrophysik,* had from the beginning adopted a more nationalistic policy, the *Astrophysical Journal* had retained its original board throughout the first world war and the years of peace.

Perhaps the greatest blow to the international character of astronomy has come through the decline in the exchange of publications. This decline started in the first world war. It was accelerated after 1939, and apparently now the prosecution of total war includes the starvation of the enemy in pure science as in all other human activities. Efforts are being made to maintain the exchange of at least a small number of publications, and to prepare for a resumption of normal exchanges after the war. The work of all astronomers has been hampered and is being hampered by such restrictions—necessary though they may be under present conditions. We shall never know to what extent progress in astronomy has been delayed, or even reversed, by personal insecurity, violent loss of life, in and out of war, fear of persecution, famine, and all the other elements of war and revolution.

As our work has become more individualistic there has been an increase in ideas and this in turn has stimulated interest in theoretical work. Prior to 1893 most theoretical work was limited to celestial mechanics. Even twenty years later a university course in "Theoretical Astronomy" meant celestial mechanics. Although some beginnings

had been made before 1893 in the theory of stellar structure by Lane, Ritter, Kelvin, and a few others, the development of the theory in the form in which we know it now falls within the last fifty years, and may well be associated with the publication of Emden's famous book "Gaskugeln" in 1907 and Eddington's "The Internal Constitution of the Stars" in 1926. Also during the past fifty years Schwarzschild undertook his investigations of stellar atmospheres and thereby gave rise to the second great trend in theoretical astrophysics. Our present organizations expect each astronomer to think for himself and to develop new ideas. Hence he must be thoroughly trained in theory. The latter decade of the period we are considering has seen an approach between theory and observation. Fifty years ago a theoretical astrophysicist would have felt out of place at an observatory and would have sought employment in a physics department. Now theoretical astrophysicists are often associated with active observatories, and the trend will probably continue.

The era preceding our own had seen a remarkable development of dynamical astronomy. G. W. Hill was recognized by the Royal Astronomical Society's award of its gold medal in 1887, G. H. Darwin received the same honor in 1892, and H. Poincaré in 1900. These researches were largely devoted to the dynamics of the solar system and that of rotating bodies. In the past fifty years dynamical astronomy has been vigorously developed, both along the older lines — by E. W. Brown's work in lunar theory, by O. Backlund in the theory of comet Encke, by H. Struve in the theory of satellites, by J. H. Jeans in the theory of rotating systems—and also along new lines—by J. H. Oort's theory of galactic rotation, by B. Lindblad's studies of dynamical conditions in extra-galactic systems and by S. Chandrasekhar's work on galactic dynamics.

The newest branch of astronomy in 1893 was stellar spectroscopy. Its founders were Sir William Huggins who obtained the first useful photographs of stellar spectra in 1863, Henry Draper whose photographs of star spectra were obtained in 1880, and H. C. Vogel who at about the same time started getting accurate radial velocities from stellar spectrograms. The tremendous development of stellar spectroscopy in the past fifty years is so well known that it is sufficient to list only a few of the high lights. Campbell undertook, at the Lick Observatory, the determination of radial velocities of all stars to magnitude 5.5 and of many fainter ones, after raising the precision of the measurements to a level (about $\pm$ 0.1 km/sec) at which it remained until a few years ago, when Adams increased the precision ten fold (about $\pm$ 0.01 km/sec) by means of the Mount Wilson Coudé spectrograph. Frost started work in stellar spectroscopy at Yerkes and concentrated mostly upon the brighter B and A stars; Belopolsky worked at Pulkovo, Küstner at Bonn, and Newall at Cambridge. J. S. Plaskett made great ad-

vances in the construction of powerful spectrographs and established Victoria as the world's most powerful observatory devoted exclusively to stellar spectroscopy.

Except for radial velocity work stellar spectroscopy has retained throughout the past fifty years the character of a pioneering science, and it still retains this character today. It is important for us to realize this when we formulate programs of research. Let me illustrate what I mean by giving a few examples. Many years ago Belopolsky at Pulkovo accidentally discovered that the star α^2 Canum Venaticorum (or Cor Caroli) has spectral absorption lines of variable intensity. He found that these variations occur in a period of 5.5 days and that some lines, later identified by Baxandall as coming from the rare-earth atom europium, are strong, when other lines, mostly belonging to chromium, are weak. Belopolsky could not explain why these changes took place, and we are still completely in the dark as to the underlying causes. The question arises how we shall proceed to solve this remarkable problem. One way would be to observe Cor Caroli on every night with the best available instruments and increase the precision of our measurements. This would be a perfectly logical procedure and it has, in fact, been carried out to a limited extent. But we must be constantly on the alert in order not to lose sight of our fundamental problem—to explain why this star has variable lines. It is unlikely that a single method of attack will suffice. For example, it may be doubted that observing the line variations every night over ten or twenty years will give us more information than we already possess. On the other hand, we are naturally curious to know whether Cor Caroli is unique, or whether other stars show variable lines. This task has been only started by the work of W. W. Morgan and of Guthnick. But it would be a mistake to spend much time on every single object thus discovered. It is more important to accumulate quickly enough data to determine approximate periods to see whether they have a tendency to cluster around five days, to decide whether the marked variability of europium in Cor Caroli has any particular significance, whether the radial velocities from the two groups of lines are also variable, whether the fact that the line CaII K Cor Caroli is abnormally weak has a more general significance. In other words, we must make brief and, at first, superficial explorations in many directions and postpone detailed measurements until we are sure that these measurements are necessary. We must formulate the problems and outline their domain before we can make headway with more elaborate studies of separate properties of single stars.

Stellar spectroscopy is perhaps the only remaining branch of astronomy where the astronomer must still function as a pioneer. We accidentally found a few weeks ago at the McDonald Observatory that ϵ Ursae Majoris, one of the stars with variable lines discovered by Guthnick, shows very strange changes in the contours of the lines which fol-

low the same period of 5.0 days, which Guthnick had found for the variations in the line-intensities. No other star is as yet known to have similar changes in the contours. The temptation was great to embark upon an elaborate spectrophotometric program of line-contour measurements. But we decided against it: at this stage of the work it is more important to make a rapid qualitative survey and probe into other stars than to give exact measurements of a phenomenon, the importance, or even the relevance, of which is as yet unknown.

In this respect the work of a stellar spectroscopist today resembles that of Lockyer or Huggins or Baxandall, and differs from that of Vogel or Campbell. I have been dwelling upon this property of research in stellar spectroscopy because I believe it is not generally understood, even among astronomers. For example, it is customary at meetings of the American Astronomical Society, for the representatives of some of the older and better established branches of astronomy to criticize the number and the qualitative character of many of the papers devoted to observational astrophysics. As one of them remarked not long ago: every time an astrophysicist takes a plate with his spectrograph he publishes two or three papers on it. Although this is an exaggeration, it does correctly represent a trend. In spectroscopy a small amount of material often suffices to answer a significant question, and if the answer can be obtained without painstaking and time-consuming measurements, the astrophysicist would be guilty of wasting his employer's money if he spent his time making useless measurements. What the critic does not know and therefore fails to appreciate is that every spectroscopist worthy of the name determines beforehand which question he wishes to answer and which observation at the telescope is most likely to give him the required answer.

We arrive at another of our characterizations of astronomy during the past half-century: observational astrophysics, in particular stellar spectroscopy, has to a large degree retained the character of a pioneering science. Although very important advances have been made in the precision of astrophysical measurement, for example in the measurement of line intensities by Dunham at Mount Wilson, by Williams and Hiltner at Michigan, by Minnaert and his associates in Holland, the bulk of the work being done now consists in a superficial exploration of the spectra and in the formulation of the problems. It is probable that this trend will continue for some years, but until vastly superior methods become available than are in use today, the tendency will be to make stellar spectroscopy truly an exact science, and to concentrate more upon the next decimal place, than is justifiable today. Beginners in astrophysics should prepare for this.

In positional astronomy the trend I have sketched took place long before our era. It is customary to describe the first half of the nineteenth century as the period when positional astronomy became an exact

science. The past fifty years have seen a continuation of this trend. The introduction of photography actually did produce an extra decimal place in the parallax work of Schlesinger and Mitchell and their numerous followers. Photographic methods have also improved the accuracy of star positions and are in constant use in the Yale surveys and in the German repetition of the A. G. zones. Meridian circles were first supplied with electrical devices, like the impersonal micrometer, to increase their accuracy, while lately successful experiments have been made to adapt the photoelectric cell to the measurement of star transits.

Galactic research has also entered upon a new stage of development, largely during the past fifty years, when Barnard and Max Wolf began to employ wide-angle lenses for the photography of the Milky Way. This remarkable development, which culminated in F. E. Ross' "Atlas of the Milky Way," together with Trumpler's recognition of general and selective absorption in the galaxy and with Shapley's studies of the system of globular clusters, led to the greatest revolution in astronomical thought since the time of Copernicus: the abandonment of the heliocentric system of the galaxy and the recognition of the true size and shape of the Milky Way. The names of the astronomers who have brought about this revolution and given us our new conceptions are so well known that I need not list them.

Although galactic research existed at least since 1837, when the first accurate stellar distance became known, extragalactic research is entirely a product of our own era, and most advances have been made in the latter half of it. Many astronomers who are active today recall the days when few experts believed that the spiral nebulae were extragalactic objects. And those who did believe so had little more to show than a "hunch." Hubble's papers "NGC 6822, A Remote Stellar System," in 1925, and "A Spiral Nebula As a Stellar System, Messier 33" in 1926, electrified the world and dispelled all doubt about the nature of these objects. The next fifty years will undoubtedly see an enormous increase in research on extragalactic systems from the theoretical as well as the practical point of view. And perhaps the crowning achievement (or near-achievement) of the era which we have been discussing is the contribution it is making to the next era in the form of the 200-inch telescope, whose most important advances are certain to be in the domain of extragalactic studies.

I cannot close this report without calling attention to two other phenomena which have developed in the past fifty years. One is pleasing, the other is distressing. The first is the ever growing participation of amateur astronomers in serious research. In America and Russia this participation has resulted in a complete revision of our attitude towards variable star astronomy. The amateurs contribute the major part of the observations. The second phenomenon is the pernicious spread of astrology. Its effects are very serious and tend to undermine public

education everywhere. In this connection I should like to relate a personal experience. About two years ago, when I appeared at the NBC studios in New York to give a radio talk on an official program sponsored by the U. S. Treasury Department, the director of the broadcast insisted that I omit from my prepared talk (which had been approved in advance by the Treasury officials) any disparaging remarks concerning astrology (which I characterized as a menace to sound thinking). The ground for his demand, the director argued, was that I was raising a controversial question on which there could be different opinions. I might add that the talk was presented as I had prepared it, but not without a hot argument which lasted to within a few seconds of the scheduled time. Astronomers have not only failed to stamp out this delusion from the public mind, but have permitted it to grow and to reach high places during the era we are discussing. Active measures will be required to stamp out the evil.

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[1] *Proc. Nat. Acad. Sc.*, **5**, 391, 1919.
[2] *A. J.*, **12**, 81, 1892.
[3] *A. J.*, **12**, 103, 1892.

YERKES OBSERVATORY, AUGUST 10, 1943.

The Brightness of Comet 1942*g* (Whipple)

By N. T. BOBROVNIKOFF

As there is some confusion in the notation of this comet I must specify that the comet to be discussed was discovered by Dr. F. Whipple of Harvard College Observatory on December 10, 1942. It was referred to as Comet 1942 *f* in the "Comet Notes" of this periodical up to June, 1943.

In the northern hemisphere the comet was an exceptionally good one for observations of its total brightness, because it was near the zenith in our latitude early in April in opposition to the Sun. I observed this comet on 58 nights mainly to check on some of my results derived in the statistical study of the brightness of comets. Altogether 128 mean values of its brightness based on 708 comparisons with stars were obtained. The details as to comparison stars, procedure, accidental, and systematic errors have been published elsewhere.[1] In this paper I want to give some of the results of more general interest.

[1] *Perkins Observatory Contributions*, No. 19, 1943.

The instruments used in this work were:

TABLE 1

	Notation	Correction
		m
Naked eye	n	+0.76
Prism binocular	b	+0.55
Galilean monocular	t	+0.58
Field-glasses	B	+0.58
2½-in. finder	f	0
9½-in. refractor	T	—0.85

The differences in the estimates between the various instruments were evaluated as follows:

TABLE 2

	m	m
b — n	+0.21	±0.02
b — t	+0 03	±0.02
b — B	+0.03	±0.01
f — b	+0.55	±0.04
T — f	+0.85	±0.08

which results in the corrections for the reduction of all observations to system f given in Table 1.

Harvard Photovisual magnitudes were taken from the *Harvard Mimeograms*. At the end of observation the magnitudes of comparison stars were obtained from Harvard Standard Region C 7.

The results of the combination of observations are presented in Table 3. Column H gives the derived magnitude of the comet, column p the weight which is simply the number of individual estimates included in the mean value of the magnitude. Finally column R_e and R_i are the external and internal probable errors of the mean values. The external probable error was derived from the residuals in the combination of observations made with different instruments after the systematic corrections explained above have been applied. For the nights when only one instrument was used no external error is given. The internal probable errors were found from the residuals in the formation of the mean values of H for each instrument. The difference between these two kinds of errors can be illustrated as follows. Suppose after the systematic corrections given in Table 1 have been applied we obtain the following values of H for one night:

	m	m
n	7.00	±0.05
b	6.50	±0.05
t	7.50	±0.05
B	6.75	±0.05
f	7.25	±0.05
T	7.00	±0.05

The internal errors are all equal to $0^m.05$, and if the observations are of equal weight the internal probable error of the combinations is, by the theory of least squares, $0^m.02$. However, if we take the mean of the values of H we obtain $H = 7^m.00 \pm 0^m.10$. This is the external probable error and in this illustration it is five times as large as the internal probable error. It is obvious that the small internal probable

errors given in this illustration are illusory and there is something wrong with the observations.

In an ideal case the average internal error should be equal to the average external error, but this seldom happens. The theory of statistical fluctuations allows us to compute the degree of tolerance permissible for this discrepancy.

In the observations of Table 3 the average external error is $0^m.054$, while the average internal error for the same observations is $0^m.035$. The ratio of these two errors exceeds unity by 54 per cent. The permissible excess is 30 per cent. Although the discrepancy is not very serious, it is clear from Table 3 that it is due to the observations in the latter part of June and in July. These observations involve comparison stars the photovisual magnitudes of which were not available and had to be obtained in a roundabout way through their color—and red-indices. The comet was then on the limit of visibility. If we neglect these observations, we obtain the average external probable error $0^m.044$, and the average internal probable error $0^m.034$ with the ratio of these two errors 28 per cent in excess of unity. The permissible excess is 29 per cent.

By correspondence three other sets of estimates of the total magnitude of this comet came to my attention. They are by J. Ashbrook,[2] Leesburg, Virginia, by Dr. Van Biesbroeck, Yerkes Observatory, and by Miss A. Farnsworth and Miss H. Pillans, Mount Holyoke College. Ashbrook's observations made with binoculars show in reference to my binocular (b) observations a systematic difference $A - b = -0^m.14$

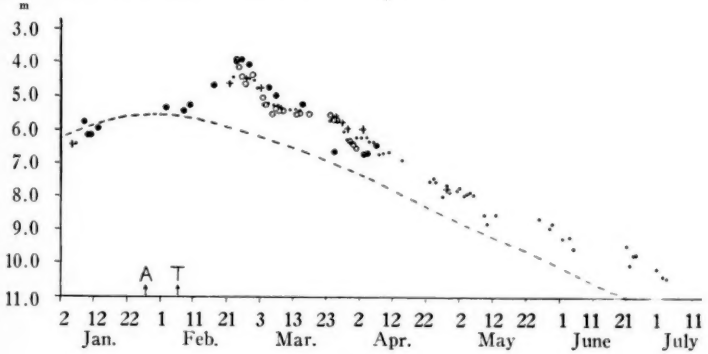

FIGURE 1

LIGHT CURVE OF COMET 1942 *g*

Crosses represent observations by Van Biesbroeck, open circles by Ashbrook, filled circles by Miss Farnsworth and Miss Pillans, dots by the author. A denotes the nearest approach to the Earth, T the perihelion passage. The curve represents the theoretical brightness adjusted to fit the first nine observations.

[2] Since published in *Popular Astronomy*, **51**, 362, 1943.

$\pm 0^m.02$. Otherwise they correspond in every detail to my observations agreeing even in the secondary fluctuations in brightness. There is no appreciable systematic difference between other observations and my own.

All these observations are plotted in Figure 1, reduced to my system f. It is seen that the agreement between the observers is excellent except for the Mount Holyoke observation of March 25 which gives the magnitude of the comet exactly 1^m lower than other observers. If we were dealing with a variable star we would reject this observation as erroneous, but with a comet it may mean a real variation in brightness.

We notice the distinctly periodic variation in the brightness of the comet with maxima on about February 24, March 24, April 23, May 23, and June 21, indicating a period of about 30 days. The deviations from the smooth curve are of the order of $0^m.2$, that is, the semi-amplitude is four times the average external probable error. This period has nothing to do with the lunar phases as the maxima of light of the comet occurred near the time of full moon. Anyway I did not observe with the Moon above the horizon. The periodic fluctuation in the brightness of comets seems to be a rule rather than an exception as I have shown in a previous study.[3]

TABLE 3

TOTAL BRIGHTNESS OF COMET 1942 *g*

No.	Date U.T.	H (m)	p	R_e (m)	R_1 (m)
1	Jan. 6.13	6.47	5	....	0.06
2	Feb. 23.08	4.41	3	....	0.04
3	28.08	4.47	8	0.05	0.06
4	Mar. 1.22	4.52	8	0.03	0.07
5	2.17	4.79	5	....	0.09
6	4.17	5.26	12	0.03	0.05
7	5.12	5.26	16	0.05	0.02
8	7.25	5.29	7	....	0.08
9	8.19	5.30	16	0.04	0.03
10	9.18	5.31	16	0.04	0.02
11	10.20	5.38	16	0.05	0.03
12	12.22	5.40	7	....	0.04
13	14.12	5.38	6	....	0.03
14	15.19	5.40	7	0.06	0.04
15	24.06	5.72	4	....	0.06
16	25.11	5.66	4	....	0.05
17	26.13	5.76	7	0.06	0.05
18	28.21	6.07	12	0.05	0.04
19	29.14	6.30	16	0.03	0.03
20	30.13	6.35	18	0.04	0.02
21	31.07	6.38	14	0.00	0.05
22	Apr. 1.10	6.28	18	0.02	0.01
23	2.09	6.26	18	0.01	0.01
24	4.21	6.24	9	....	0.03
25	5.25	6.32	18	0.03	0.02
26	6.09	6.33	18	0.02	0.02
27	8.15	6.70	8	0.07	0.05
28	9.21	6.70	10	0.01	0.04
29	11.26	6.67	18	0.02	0.02

[3] *Perkins Observatory Contributions*, No. 16, 1942.

No.	Date	U.T.	H m	p	R_e m	R_1 m
30		15.40	6.90	15	0.03	0.03
31		23.10	7.54	9	....	0.03
32		24.10	7.47	4	....	0.07
33		25.18	7.56	16	0.03	0.04
34		27.10	7.99	12	0.09	0.03
35		28.14	7.63	14	0.02	0.04
36		29.13	7.85	18	0.08	0.03
37	May	1.14	7.81	24	0.06	0.05
38		2.14	7.77	26	0.07	0.02
39		3.30	7.99	8	0.01	0.06
40		4.26	7.90	31	0.08	0.02
41		5.16	7.88	18	0.01	0.01
42		6.18	7.90	33	0.06	0.02
43		9.36	8.54	19	0.04	0.06
44		10.07	8.80	4	....	0.09
45		13.34	8.48	18	0.02	0.02
46		26.18	8.63	5	0.06	0.04
47		29.15	8.91	14	0.08	0.04
48		30.24	8.78	15	0.08	0.03
49	June	2.15	9.24	12	0.11	0.03
50		4.21	9.20	14	0.03	0.05
51		5.15	9.52	3	....	0.07
52		21.15	9.43	8	0.12	0.03
53		22.13	10.02	9	0.20	0.05
54		23.14	9.17	11	0.03	0.04
55		24.14	9.71	9	0.14	0.04
56		30.14	10.17	7	0.15	0.03
57	July	2.13	10.38	4	0.08	0.04
58		3.17	10.41	4	....	0.05

We now come to the most interesting part of the light curve. The dotted line on the diagram represents the theoretical brightness of the comet computed on the assumption of a variation in brightness inversely proportional to $\Delta^2 r^4$. Since the major part of the apparent variation in brightness was due to the variation of the geocentric distance, the exponent of r does not influence the run of the curve to a great extent, and if we assume a variation inversely proportional to r^2 or r^6, the general character of the curve will be the same. Since the nearest approach to the Earth occurred at about January 28 ($\Delta = 0.43$, denoted A in the diagram), and perihelion on February 6 ($q = 1.36$, denoted T), the maximum theoretical brightness should occur between these two dates, and nearer to A than to T.

The curve is so adjusted as to satisfy the first nine observations, and the theoretical brightness can be expressed thus:

$$H = 5^m6 + 5 \log \Delta + 10 \log r.$$

We notice that by shifting the curve upwards by $1^m.0$ we can satisfy the observations between March 3 and July 3 very well, that is, these observations can be represented as follows:

$$H = 4^m6 + 5 \log \Delta + 10 \log r.$$

But this curve will not satisfy other observations. Those at the end of February will be 1^m above the curve, and those between January 5 and February 10 by an equal amount below the curve.

It appears, therefore, that the brightness of the comet underwent two rather sudden increases on about February 10 and February 23. After the second increase in brightness, the comet remained "permanently" brighter by 1^m as compared with the January observations.

What happened then, cosmically speaking, on about February 10 and February 23? It is natural to turn our attention to the Sun. We find[4] that in spite of a minimum of solar activity two groups of sunspots large enough to be seen without a telescope crossed the central meridian of the Sun on February 11.7 and 25.7, but no magnetic storms were recorded. Since the comet was nearly in opposition these spots would be also near the central meridian as seen from the comet.

The absence of magnetic storms need not disturb us, however. The luminosity of a comet is controlled by the ultra-violet radiation of the Sun, and the connection between the magnetic storms and the ultra-violet radiation is not very close. What we want to know is whether any variation in the ultra-violet radiation of the Sun, such as that evidenced by the Dellinger Effect, has been recorded, but at the present time I have no data on this question at my disposal. At any rate we know so little about the luminosity of comets that even if the Dellinger Effect was observed on these dates the interpretation of the comet's behavior is by no means simple.

Such an interpretation has been attempted by N. Richter[5] in connection with comet 1939 *d* which suddenly increased in brightness on April 21, 1939. A strong Dellinger Effect was recorded on that date and also on the following day, but the comet on April 22 was decreasing in brightness. The author had to assume that too much of the ultra-violet radiation may decrease the luminosity of a comet by increasing the ionization of molecules in the head of the comet.

Any theory showing the connection between the activity of the Sun and the luminosity of comets must take into account some very troublesome facts. Attempts to establish a correlation between sunspots and variation in the brightness of comets have uniformly been failures.[6] Some comets, such as 1925 II (Schwassmann-Wachmann Per.) are strongly variable[7] at a distance of more than five astronomical units, while other comets at one-tenth of that distance from the Sun show no evidence of variation. Finally, the behavior of components of comets that broke up (such as Biela, 1889 V and 1915 II) is very difficult to explain. In these comets some components were variable while others were not. In Comet 1889 V (Brooks) the solid angle at the surface of the Sun subtended by the components was but a few minutes of arc. A beam of ultra-violet radiation from the Sun affecting one component and missing another is hardly imaginable.

[4] Mount Wilson Data, *Publ. A. S. Pac.*, **55**, 112, 1943.
[5] *A. N.*, **269**, 103, 1940.
[6] See for further reference *Perkins Obs. Contr.*, No. 16, 1942.
[7] G. Van Biesbroeck, *Reviews of Modern Physics*, **14**, 163, 1942.

All these difficulties can be obviated by the hypothesis that the physical state of some comets (and even of the same comet at different times) is vastly different from that of others in spite of the uniformity of their spectra, but there is much work to be done yet before any conclusions on this matter are reached. What we record in observations of brightness or of spectrum is only the final result of a very complex physical process involving very complex molecules.

The period of about 30 days in the secondary variation in the brightness of the comet need not correspond to the 25-day period of the rotation of the Sun. Since the comet's motion was direct the synodic period of the rotation of the Sun as seen from the comet should be somewhat longer than the sidereal period of the rotation of the Sun. But the comet was not moving with a uniform speed nor in the plane of the equator of the Sun, and therefore allowance should be made for these factors. It is obvious that no further investigation along these lines is profitable until we locate the source of the disturbance on the surface of the Sun.

It would be of interest to compare the spectrum of the comet before and after these outbursts, but I have no expectations on this score. Comet 1892 III (Holmes Per.) suddenly increased in brightness by several magnitudes without the least change in its spectrum. The same can be said of half a dozen other comets.

Experience with Comet 1942 *g* proves two things. Valuable information on cosmic relationships can be obtained with very simple instrumental means provided sufficient care is taken in observing the brightness of the comet. Secondly, comets must be observed continuously. It is always when you let them go for a while that something quite unexpected happens to them.

The readers of this article will contribute much to the elucidation of the behavior of this comet if they publish their observations on its brightness, especially for the period December 10, 1942, to February 23, 1943.

PERKINS OBSERVATORY, AUGUST 23, 1943.

Note added in proof, October 25: The writer has recently received Astronomical Circulars of the Academy of Sciences of U.S.S.R., Nos. 12 and 13, containing estimates of the brightness of this comet by D. J. Martinoff (Engelhardt Obs., Kasan), A. M. Bakharev (Stalinabad) and G. A. Lange (Kitab).

The systematic corrections to these observations to reduce them to my system f have been evaluated as follows:

Observer	No. of Obs.	Correction
Martinoff	13	$+0^m43 \pm 0^m04$
Bakharev	12	$+0.87 \pm 0.08$
Lange	8	$+0.97 \pm 0.04$

Twenty-two of these observations were made between January 14 and February 11 determining the behavior of the comet in the region

where only three observations have been used by me for the construction of the light curve (Figure 1).

This additional material puts beyond any doubt the existence of the 30-day period in the variation of the brightness of the comet. The comet was brighter by one magnitude on January 25 than on January 19 and was continuously decreasing in brightness from January 25 to February 8. We may assume a maximum of light on January 25, although there is still a gap between the observations of January 19 and January 25. This gap is not likely to be filled as full moon occurred on January 21.

The semi-amplitude of the fluctuation of light for the January outburst was $0^m.5$, and for the February outburst $0^m.7$. After February the semi-amplitude gradually diminished to about $0^m.2$ in June. The similar gradual diminution of the amplitude of the periodic variation in light was found by the author for Comets 1902 III and 1912 II (*Perkins Obs. Contribution,* No. 16, 1942).

Measuring the Light from the Stars

By L. V. ROBINSON

When one comes to consider systematically and with extraordinary precision how much light reaches the eye from a star, he is at once challenged by a multitude of difficulties. First, he must have a unit for comparison; and this necessitates the choice of a scale. No question is raised in deciding upon a magnitude scale; and selecting the interval, or intervals, of brightness between two or more comparison stars, the investigator seeks to reduce such to a *magnitude* scale. It is here that his difficulties begin. He may have been so fortunate as to choose all his comparison stars of the same color. If he has other stars to compare with these, he may by some rare circumstance have chosen his comparison stars of the same color as the other stars involved. But what is he to do if his selections are not of the same color? Even though his data relating to apparent brightness increase in value as he adds more stars to the list and although the interval of brightness between the two comparison stars chosen might otherwise be satisfactory, the most challenging fact yet remains that the stars differ greatly in color as well as in apparent brightness.

It is therefore necessary to compare the light received from the stars both qualitatively and quantitatively; and psychologically by means of the naked eye it is impossible to separate these effects—of color and of amount. This fact is further emphasized, and the problem further complicated thereby, when another observer offers his contribution to the data. The first observer, for instance, may estimate that a red star is of the same brightness as a white star which he may have chosen for comparison; but the second observer does not see it that way.

He—the latter—estimates that the red star is either brighter or fainter than the white one; and there is no basis for agreement between the two observers.

Estimates of the color of the stars are generally based on prior knowledge of spectral type, and spectral type depends mostly upon temperature. As is the case of an iron bar, a white star is hotter than a red one; and the hottest stars are about twenty times hotter than the coolest stars known. Although the temperatures of the stars, as assumed from their spectral types, are not so well known as might be desired, it is difficult to devise a more convenient scheme than to take temperature as determined by spectral type. At least this affords a convenient basis for qualitative studies based on color and for investigating the effects of color in estimating relative brightness.

Over an interval of four years spent at Harvard, the writer made a special study from photographic plates of the variations in brightness of 106 Cepheid variables. In this study—the results of which were published in *Harvard Annals,* **90**, No. 2, 1933—it was, of course, necessary to reduce the observations to a magnitude scale; and under ordinary circumstances, this is no easy task. Especially is this true in any study making use of photographic plates. On the photographic plate, for instance, stars at different distances from the center can be compared only in the face of considerable risks. If at all possible, therefore, magnitudes should be obtained from stars in the immediate neighborhood of the variable and its comparison stars. In this connection, the Harvard photometric survey—which extends over the whole sky and is, for the most part, the work of Professor Pickering and Professor Bailey—suggests itself as a possible source of photographic magnitudes.

The problem now resolves itself into one of preparing these visual magnitudes for photographic work; and here we are confronted with two difficulties of the first rank. In the first place, how is one to determine what *is* the visual magnitude of a star when there is hardly a chance for agreement between two given observers. Sufficient evidence of this fact is obvious from Fig. 1, where the mean difference between the estimates of Pickering and Bailey (Pickering *minus* Bailey) has been plotted against the reciprocal of the mean "effective" temperature inferred from spectral type. In the second place, due to the fact that the photographic plate in general is sensitive to shorter wave-lengths of light than the human eye, the photographic plate shows the blue stars brighter and the red stars relatively fainter than the naked eye sees them. This difference—photographic minus visual magnitude, which is known as the *color index*—must therefore be examined.

The question as to how the first of the above difficulties is to be overcome immediately suggests itself. Obviously, without giving more weight to one observer than to the other, the visual magnitudes of one

observer can be taken as the standard. Then, knowing the temperature as inferred from spectral type, the observations of the other can be reduced to that standard by making use of Fig. 1. This is exactly what was done, the reductions being made to Pickering's magnitudes solely for the reason that more of the observations were made by Pickering. Then, for the cases where observations of both Pickering and Bailey

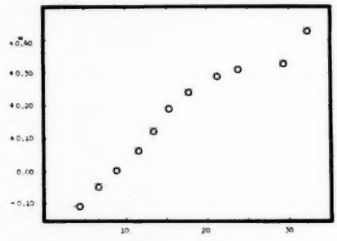

FIGURE 1

THE EFFECTS OF TEMPERATURE AND COLOR UPON ESTIMATES OF VISUAL MAGNITUDE

The abscissae are values of $10^5/T$ and the ordinates represent the differences of Pickering's from Bailey's estimates.

are available, after the corrections indicated by Fig. 1 have been added to Bailey's magnitudes, the means between the two sets of magnitudes are taken. Where the observations are due to Bailey alone, it is of course only necessary to add the corrections indicated by Fig. 1 in order to reduce them to Pickering's standard.

Even with this degree of care, the data cannot be claimed to be free of sources of systematic error; for the so-called Purkinje effect remains yet to be considered. For two reasons, however, this effect seems to have been reduced to a minimum. In the first place, assuming that each observer estimates the brightness of red stars systematically fainter than he would white stars of the same apparent luminosity, as the Purkinje effect presupposes, then the manner in which the visual data are reduced seemingly minimizes the effect as it enters into the determination of photographic magnitudes. This follows from the fact that the color index of A0 stars is taken to be zero for faint stars as well as for bright ones. In the second place, had the primary purpose of the investigation been centered upon an intensive study of the systematic differences between the observers, then more time and more stars necessarily would have been utilized in the study.

Supposing now that all the required corrections have been made to reduce the visual magnitudes to a satisfactory standard system, another major difficulty remains to be overcome: How is one to make use of dependable *visual* magnitudes to obtain reliable *photographic* magnitudes? The immediate answer is, of course, to add the color index to the values of the visual magnitude. But color index is a relative term.

Whose eye is to serve as a standard for determining visual magnitudes and what photographic plate exposed through what telescope is to serve as the standard for photographic magnitudes? As has already been indicated, for the program devoted to the study of the light variations of Cepheids at Harvard, the writer chose the visual measurements of Professor Pickering. The plates, moreover, for all practical purposes were of the same quality and, for the most part, taken through two telescopes—two short-focus cameras one inch in aperture—as nearly alike as is humanly possible to construct.

Assuming the photographic magnitudes of A0 stars to be the same as their visual magnitudes, a photographic magnitude scale can therefore be set up; and any deviation from this scale will be the color index. Provisional values corresponding to the range of spectral types, as obtained over a limited region of the sky, are further improved by applying such corrections as will make the color index of A0 stars zero in all parts of the sky and for all magnitudes.

But the color indices obtained here are of a very special sort. It must not be overlooked that they only represent the differences, m_p—m_v, between Pickering's *visual* magnitudes and the *photographic* magnitudes peculiar to the telescopes and the photographic plates used in this investigation. Such values of the color index cannot claim serious consideration under other conditions.

From theoretical considerations it is not difficult to show that color index should be very nearly proportional to the reciprocal of the temperature. In other words, assuming the temperature to be known, the color index plotted against the reciprocal of the temperature should

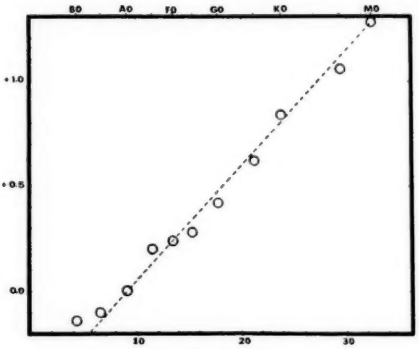

FIGURE 2

THE RELATION OF COLOR INDEX TO TEMPERATURE

The abscissae, at the bottom, are values of $10^4/T$ and, at the top, spectral type; and the ordinates are values of the color index.

give a straight line, as indicated in Fig. 2. The temperatures of the stars, it should be stated, can be had from other data and are found to be closely related to spectral type. But the problem is yet marred by the fact that a dwarf star of spectral type G0, say, is hotter than a giant of the same spectral type. For such an investigation as is here described, it is therefore fortunate that few dwarf stars are to be expected from random selections. Dwarfs do not appear among the brighter stars in the sky for the simple reason that stars intrinsically faint cannot be seen at great distances from the Earth and Sun. In fact, among the stars used here to obtain color index—and from color index, photographic magnitudes—not one dwarf was found.

Using the temperatures given by Russell, Dugan, and Stewart ("Astronomy," p. 753) and plotting color index against the reciprocal of the temperature, Fig. 2 was therefore obtained. The broken line which best fits the data has for its equation,

$$I = (5500°/T) - 0.50,$$

where I is the color index and T is the effective temperature. To reverse the problem, then, is to have a method of measuring the temperatures of the stars by first determining their color indices; and this is one important means by which the temperatures of the stars are estimated. The accuracy of the method depends, of course, on the accuracy of the visual and photographic magnitudes of the stars the temperatures of which are required. A change of visual observers, of telescopes, or of sensitivity of photographic plates, in turn, demands a change in the constants involved in the above formula, the general type of which is

$$I = (a/T) + b.$$

Such is but a bare outline of the more general problems associated with the determination of photographic magnitudes by the use of visual magnitudes; and no one who has been confronted with the task of obtaining reliable photographic magnitudes needs to be convinced of the difficulties underlying his problem. In the first place, the photographic observer is without any very direct means of ascertaining, for instance, that one star is twice as bright as another by a simple comparison of their images on the plate. Moreover, if he is given a sequence of stars the photographic magnitudes of which are presumably known, he still has no reliable standards for comparison unless the sequence is located in the immediate neighborhood of the stars the photographic magnitudes of which are to be determined. This follows from the fact that, in general, the scale is steeper near the edge of the plate than near the center, that all the stars on the plate cannot be in focus at the same time, and the fact that blue stars and red stars are affected differently by focus and by distance from the center of the plate.

The above discussion, therefore, does not as yet contain the last word as to the technique of obtaining reliable photographic magnitudes from

visual ones. Any investigator is eternally in the presence of human error. To the errors inherent in the visual magnitudes themselves, we have necessarily affixed others—and larger ones, probably—when we seek photographic magnitudes by merely adding color index. At best, we have only the mean value of the color index presumably characterizing a given spectral type, and there are certainly considerable deviations from the mean. When it is remembered that each of the circles in Fig. 2 represents the mean of many observations, the validity of this statement becomes apparent. The deviations from the mean are due, first, to individual differences among the stars and, secondly and to a less extent perhaps, to accidental errors in estimating the spectral class of the stars.

Inasmuch as the photographic magnitudes obtained by the above methods are only provisional, and by no means perfect, further refinement will therefore be sought. Suppose now that for comparison purposes a series of exposures of two or more stars, say, are made on a small area of a photographic plate, the first exposure being the shortest and each one successively doubled as the plate is shifted slightly between exposures. Thus one has a graduated scale of star images, the difference between them being in effect slightly less than a magnitude—since a magnitude represents a factor of about 2.512 in brightness. By means of this graduated scale of star images, one may set up an arbitrary scale—in the present case, based on two different series of star images

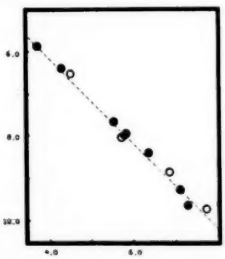

FIGURE 3

PHOTOGRAPHIC MAGNITUDES AS MEASURED BY AN ARTIFICIAL SCALE

The artificial scale (abscissa) is determined from a series of images for each of two stars with times of exposure taken according to a constant ratio. Designating such images on the small section cut from the photographic plate as 0, 1, 2, . . . and 10, 11, 12, . . . and establishing the relationship between them, two independent comparisons can be made with star images on any photographic plate in terms of the artificial scale. Once a relationship between the scale and photographic magnitude (ordinates) is established, the photographic magnitudes of all stars in the region can be obtained from their scale measures. This relationship can be established by choosing a few stars for which photographic magnitudes, obtained by adding color indices to Pickering's visual magnitudes, are available. The open circles in the graph are from sources other than the Harvard data and therefore carry lower weights in the determination of the broken line representing the scale relations.

the relation between which is known—with which the photographic brightness of a group of stars may be compared. Fig. 3 represents the results of such a comparison for a certain region. Inasmuch as both the arbitrary scale (the abscissae) and the provisional photographic magnitude scale (the ordinates)—obtained from the visual magnitudes of stars in the field, as outlined above—represent geometric ratios in the amount of light exposed, a linear relation between the scales should exist. Obviously, this is true; and from this relation, the arbitrary scale (the abscissae) may be reduced to magnitudes.

In general, two independent comparisons between each star image and each of two images on the graduated scale are made. Inasmuch as the relation between the images on the graduated scale is known, each star is therefore measured, in effect, four times and the mean between the four measurements taken. This was repeated until four plates, each of the best quality, had been measured. Hence, the abscissa for each star in Fig. 3 represents the mean of sixteen independent comparisons with the graduated scale. Thus, aside from relatively small accidental errors in reduction, the photographic scale of magnitudes, obtained from A0 stars, is the same as the visual scale.

Suffice it then to say at this point that as yet we have failed to set forth any technique by which *all* the light energy from a star can be measured. One eye is sensitive to a certain range of wave-lengths of light and another is sensitive to another range. A given photographic plate, on the other hand, is sensitive to one range, and another photographic plate of a slightly different quality is sensitive to still another range. Moreover, one telescope permits a selective range of wave-lengths to get through, either to the unaided eye or to the photographic plate, while another telescope yields still another selective range of wave-lengths. How is one to know then exactly how two stars of different color compare in total output of light?

The answer to this last question is to be found in accurate determinations of bolometric magnitudes—that is, in measuring the total radiation of *all* wave-lengths emitted by the stars—insofar as this ambition may be realized. Unfortunately, our information as obtained from direct measuerment is quite scanty here. If experiments may be trusted, however, this end may be reached indirectly with a considerable degree of accuracy. This is done by making use of a formula due to Hertzsprung,

$$m_b = m_v - 2.3\ (14{,}300/T)^{0.93} - 10\ \log\ T + 42.92,$$

in which m_b is the bolometric magnitude, m_v the visual magnitude, and T the effective temperature. It is to be emphasized, however, that in making use of this formula we still are confronted with the question as to whose eye and whose telescope are to be utilized in determining m_v. In the absence of an answer to this question, we shall then be content with a first approximation to the truth.

Assuming Hertzsprung's formula and the formula for color index,

$$I = (5500/T) - 0.50,$$

it becomes possible therefore to eliminate m_v between the two formulae and to obtain a third which, when the temperature is known, is applicable to photographic magnitudes. This equation is

$$m_b = m_p - (5500°/T) - 2.3\,(14{,}300°/T)^{0.93} - 10 \log T + 43.42.$$

Given this formula, which at best applies only to such photographic material as was used by the writer in the study of the 106 Cepheids referred to above, we shall undertake the problem of obtaining bolometric magnitudes for as many of these stars as possible.

Inasmuch as the above formula presupposes a knowledge of the temperature, which is not directly known, it becomes necessary to rely on available spectral types for such data. Fortunately, for 32 of the 106 bright Cepheids, the variations of spectral type with light phase are known with sufficient accuracy to justify the undertaking just outlined. The spectral data are the estimates of Shapley, of Miss Cannon,

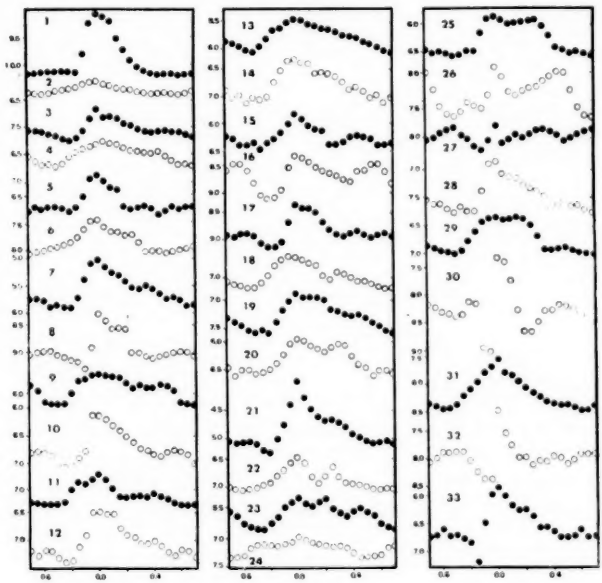

FIGURE 4

THE COMPUTED BOLOMETRIC MAGNITUDE CURVES FOR THIRTY-THREE CEPHEID VARIABLES

The abscissae are the phases expressed as fractions of the periods, and the ordinates are the bolometric magnitudes computed from photographic magnitudes and spectra. The stars may be identified from the Table.

and of Mrs. Mayall, and, in particular, the more recent data published by Mrs. Mayall and Mrs. Baker.

In addition to the 32 stars just mentioned, it is also possible, insofar as the spectral data are concerned, to add two others. One of these is Y Sagittarii, the light variations of which were studied on Harvard plates by ten Bruggencate. For the other, δ Cephei, a light curve was obtained by the writer; but, due to uncertainties underlying magnitude determinations of bright comparison stars, this star was rejected. For the same reason, SU Cassiopeiae seemed open to suspicion. Hence, with the sacrifice of the claim to perfect homogeneity and with full recognition of its dubious position here, the photovisual light curve obtained by Mrs. Gaposchkin has been substituted for the photographic light curve of this star. As Mrs. Gaposchkin points out, however, the chief difference between the two consists in the difference in range.

The variations in bolometric light were therefore computed for 33 Cepheids, and the results are shown in Fig. 4. The light curves here can be identified from their ordinal numbers in the table.

TABLE

Star	Log P	Ptg. Range m	Bol. Range m	Star	Log P	Ptg. Range m	Bol. Range m
1. RR Leo	−0.345	1.06	1.11	18. BB Sgr	+0.822	0.96	0.57
2. SU Cas	+0.290	0.32	0.25	19. U Sgr	0.829	1.16	0.72
3. U TrA	0.410	0.74	0.58	20. R Mus	0.876	1.10	0.68
4. SZ Tau	0.498	0.60	0.40	21. W Sgr	0.880	1.81	1.28
5. R TrA	0.530	1.03	0.73	22. W Gem	0.898	1.09	0.58
6. SS Sct	0.565	0.82	0.61	23. S Sge	0.923	1.04	0.58
7. RT Aur	0.572	1.28	0.86	24. YZ Sgr	0.980	0.79	0.37
8. V Lac	0.636	1.24	1.00	25. S Mus	0.985	0.98	0.73
9. T Vul	0.647	0.88	0.54	26. Z Lac	1.037	1.36	1.11
10. S Cru	0.671	1.35	0.94	27. XX Cen	1.040	1.26	0.46
11. AP Sgr	0.704	0.98	0.53	28. TT Aql	1.138	1.68	0.92
12. V Cen	0.740	1.30	0.93	29. Y Oph	1.234	0.86	0.67
13. Y Sgr	0.761	1.00	0.60	30. SZ Aql	1.234	2.56	1.71
14. R Cru	0.765	1.37	0.79	31. RY Sco	1.308	1.17	0.92
15. S TrA	0.801	1.25	0.63	32. WZ Sgr	1.339	2.17	1.25
16. RR Lac	0.807	1.15	0.79	33. U Car	1.588	1.95	1.37
17. XX Sgr	0.808	1.16	0.77				

As will be seen also, the arrangement is in order of period. Although it is realized that this method of obtaining bolometric magnitudes at best is only a first approximation, there are yet a few general observations which seem more or less reliable, and perhaps significant. These are: (1) the bolometric range averages only about two-thirds of the photographic range, whence it is concluded that about one-third of the variations in photographic light are due to changes in color; (2) the same correlation between period and form of light curve, observed first by Hertzsprung and—for photographic light variations—established by the writer, persists for the calculated bolometric variations; (3) this correlation rests, for the most part, upon the behavior of the secondary wave in the light curve; and (4) the secondary wave is apparently

more pronounced in the bolometric than in the photographic light curve.

In conclusion, it must be pointed out that, if, with reference to the comparative brightnesses of the stars, we have learned much, we have attained such successes in spite of our crude methods of measuring the light received from the stars. The methods ordinarily used do not take into account the effect of color to an extent to be desired. The use of more refined methods for analyzing, both quantitatively and qualitatively, the light from the stars is therefore an outstanding problem for the future. It is true that such methods are known and are used in isolated cases, but the fact remains that they do not as yet lend themselves readily to the rapid analyses required for intercomparisons between large numbers of stars. Then, we may perhaps ask: How are we to know the nature of the stars except by intercomparisons?

MARSHALL COLLEGE.

Notes on a Forgotten Episode

By THEODORE R. TREADWELL

"The lamented author of the following treatise in early youth developed talents for Practical Astronomy so extraordinary, as to leave no doubt, that, had his life been spared, he would have risen to a rank among the first astronomers of the age. . . Science will long mourn the loss of a youth signally endowed with that rare assemblage of qualities essential to the structure of a great astronomer in which are united the refined artist and the profound mathematician." So wrote—one hundred years ago—Professor Denison Olmsted of his young protégé and collaborator, Ebenezer Porter Mason. Professor Olmstead held the chair of Natural Philosophy and Astronomy at Yale from 1825 until his death in 1859.

Ebenezer Porter Mason was born in beautiful Washington, Connecticut, in 1819. His father, Stephen Mason, was pastor of the Congregational Church; his mother, Elizabeth Brown, died when Ebenezer was only three years old. Later his father remarried and the step-mother reared the child with as much care as his own mother might have lavished. The church in whose shadow he was nurtured is still standing in a lovely setting on the village Common, but the parsonage in which he was born was razed many years ago to make room for modern buildings of a famous school for boys. Owing to the vicissitudes of a rural pastor's life in those days and partly through solicitude for the boy's health, he was sent when eight years old to live with an aunt, Harriet Brown Turner, in Richmond, Virginia. His early training, though strongly colored by the strict religious views of his father and his Uncle Turner, who was also a Congregational minister, seems to have been fairly liberal and quite eclectic, including mathematics, science, mythology, and ethics. In his earliest childhood he evinced intense

interest in natural phenomena and, while very young, developed extraordinary skill in penmanship and drawing. One hundred years after his death the legend of his skill persists. One elderly lady, a relative of the Mason family, remarked when his name was mentioned, that "he was the boy who could draw a perfect circle free-hand." During his last illness he wrote for the amusement of a young cousin, the Lord's Prayer on a single line three inches long, in letters like micro-engraving.

On the voyage from New York to Richmond he showed great curiosity about the working of the steam engine, no doubt an intelligent interest, for he had already studied "The Steam Engine" by Stewart, and Wood's "Railroads." The next year he read Bacon's "Novum Organum" and it was at this time he began the informal study of astronomy under the tutelage of Mrs. Turner. Having been fitted with glasses to correct astigmatism, he was able to see the stars in all their beauty for the first time. There was available an English text on astronomy and a celestial globe from which he made star maps, soon learning the constellations and giving considerable attention to the mythology associated with the stars. About this time the Reverend Mr. Mason severed his connection with the Washington church and when young Mason left Richmond in 1830 he rejoined his family in Nantucket. There he assisted in the local school, teaching arithmetic which he had mastered as a pastime in his spare moments. Already his father and others had recognized his extraordinary talents,—his mental acquisitiveness which kept him constantly investigating the details of the mysterious and beautiful world,—the intense fascination which books of all kinds held for him,—and noticeable gifts of concentration and comprehension,—all these indicated the genius of mature years, indeed so much so that his parents worried about his health and tried, with little success, to induce him to spend less time with his beloved books and engage in outdoor pastimes with young companions.

When he was thirteen his father sent him to the Ellington school near Hartford for a year, where his chief study was Latin. He finished Virgil's Aeneid that year and acquired a reading knowledge of French as well. At sixteen (1835) he entered Yale, attracting the attention of one of the examiners, Professor Denison Olmsted, by the excellence of his entrance examination in mathematics. A few weeks later, happening to overhear Professor Olmsted arranging an evening session for observation with a small telescope, he begged permission to be allowed to attend. Thus began a friendship and close association which lasted until the untimely death of Mason. He immediately began the study of astronomy as an extra-curricular activity, not being admissible to any course in the subject in his Freshman year. He used a 6-inch Herschelian reflector borrowed from a classmate with which he made and recorded systematic observations of Jupiter's moon's, the rings of Saturn, occultations, and various other phenomena. However, by the

end of the year these desultory observations had become too familiar to retain his interest, and he began to devote himself seriously to binaries and nebulae.

During his Sophomore year, in March, 1837, a remarkable cluster of sunspots appeared. At Professor Olmsted's suggestion he made and recorded daily observations of these. Mason produced, according to Professor Olmsted, "A journal of observations accompanied by a magnificent drawing, which for beauty of penmanship and extreme finish in the delineation of the minutest changes of figure or dimensions in the spots, has never been surpassed by any astronomical drawings with the pen which I have ever seen. It was on a large sheet and exhibited seven views of the clusters as they varied their appearance from day to day. . . I was not more pleased with the beauty of the drawings than astonished on perusing the manuscript to recognize in one so young such powers of observation and reflection. The original drawings are deposited among the astronomical apparatus of Yale College." Unfortunately a recent search in the archives of the Astronomy Department was unsuccessful, the papers having disappeared, probably many years ago. In this same year Mason calculated the time of an eclipse of the Moon for recreation, not yet having had any formal course in astronomy, nor had his class yet had the requisite preliminary training in mathematics. In his Senior year Mason became an unofficial assistant to Professor Olmsted, often relieving him of evening work by managing the college telescope with groups of students who observed the more familiar celestial objects.

Amateur telescope making seems a comparatively recent activity, yet we find Mason and his friends grinding and figuring reflectors (speculum metal), which, according to report, gave excellent results. Several were made of six to seven inches in diameter, both Newtonian and Herschelian, and one of Mason's letters gives an account of an evening in his room devoted to figuring a mirror with two classmates, each one taking his turn in the tedious task until a late hour, with frequent trips outdoors to make a hasty test sight of the stars, for the Foucault test was then unknown. This was an active and happy period in his life although symptoms of tuberculosis already had caused anxiety among his friends.

After Mason's graduation in 1839 he assisted Professor Olmsted by proof-reading the manuscript of a new edition of the "Introduction to Astronomy" and at the professor's request began a supplement for the same entitled "Practical Astronomy," an observer's handbook and manual, with directions for the use of astronomical instruments and numerous methods for making calculations. He worked diligently on the manual for several months but during the summer of 1840 Mason's health had deteriorated so severely that Professor Olmsted and other friends were alarmed, and when an opportunity came for him to join

a commission to survey the disputed Maine-Canadian boundary, it was thought that life in the open air might restore him to health. Accordingly he proceeded into the wilderness with the expedition. The work was strenuous and fatiguing in the extreme, certainly not suitable for one afflicted by the ravages of tuberculosis, yet when he returned to New York late in October he seemed in a most optimistic frame of mind. He remained there correcting proofs of the "Practical Astronomy" and reducing the observations of the surveying expedition. But a sudden turn for the worse forced him to give up all thought of work and he accepted Professor Olmsted's invitation to join him in New Haven. There he finished the last pages of his supplement and in December he boarded the boat for Richmond to join his uncle, the Rev. J. H. Turner. Eleven days after reaching his uncle's home he died, December 26, 1840.

Besides two papers published in the American Philosophical Transactions (1840-41) the "Practical Astronomy" appearing as a supplement to Olmsted's textbook is the only record of Mason's achievements in astronomy. Examination of this work would seem to justify Olmsted's estimate of Mason's ability. It has chapters on the telescope, minor instruments, the transit, time, calculations of eclipses of the sun and moon, reducing occultations, latitudes and longitude, and many problems illustrative of the methods of calculation. The style is lucid, the directions detailed, showing an insight into the usual difficulties encountered by the beginner. As Olmsted says, "Young Mason, by an extraordinary flight of genius rose so rapidly to the highest walks of astronomy, that various expedients, which he had employed to surmount the obstacles that successively stood in his path, were still fresh in his memory, and he was therefore able to take the young aspirant by the hand and conduct him through the dark and intricate way he himself had recently threaded with such wonderful success."

For a youth of twenty-one whose training in astronomy and the use of instruments was almost entirely an extra-curricular activity, it seems an extraordinary accomplishment, a work of genius exerting an influence for many years on the teaching of astronomy. It should entitle its author to at least a small niche in the astronomical Hall of Fame.

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The Total Solar Eclipse of January 25, 1944

By LEWIS J. BOSS

Records of the early total solar eclipses in this series seem to be lacking. From the first total eclipse of the series on March 24, 1457, until December 12, 1871, no scientific observation upon any of the twenty-two repetitions seems to have been made. This is probably due to the fact that the path of totality in these cases lay largely over the Pacific and Indian Oceans and across wild and uninhabited land.

At the December 12, 1871, eclipse, one of the first observations of the "flash spectrum" was made by Herschel and Lockyer, the path of totality passing at that time over India, Ceylon, and Northern Australia. These observations, together with photographs of the corona obtained from widely separated stations yet showing the same details in the coronal streamers, finally proved that the corona is truly a solar appendage. This eclipse is noteworthy in that a "slitless spectroscope" was used for the first time in observing it.

At the December 22, 1889, return of this eclipse it barely escaped being visible in New England, the path of totality passing just east of Cape Cod, Massachusetts.

In view of the fact that most astronomers are busy with war work of one sort or another it seems doubtful if any pretentious expeditions will be sent to South America to observe it. Should any astronomer, however, be so situated that he can leave the country in war-time to make observations his trip will not be without precedent.

In 1780, Harvard University, with the aid of the Government of the Commonwealth, fitted out the Lincoln Galley, placed a party of nine under the direction of Professor Samuel Williams and sent them to Penobscot Bay to observe the total solar eclipse of October 27 of that year. The area covered by the track of totality being in British hands, Professor Williams had to obtain the permission of the English officer in charge to land his party. This was given but the time allowed was so brief that not much work of scientific value could be done. Then, also the maps of the region were so inaccurate that the location selected was just on the edge of the shadow path. So it seems that, even in those strenuous and pressing times, the Government had time to devote to the cause of science, although engaged in fighting a bitter and distressing war.

Herewith are given the dates of the beginning and ending of the partial eclipses, the date of the first central, the date of change from total to annular, date of the middle eclipse of the series, dates of total eclipses of longest duration of totality on the central line, date of last total eclipse, and date when the series ends.

1. The first partial eclipse of this series took place on August 29,

1114. All eclipses of this series take place at the moon's descending node (☋).

2. The first central, and total, eclipse of the series, occurred on March 24, 1457.

3. The middle eclipse of the series took place on November 8, 1817. Since the eclipses of this series take place at or near the moon's descending node each successive path of totality moves gradually northward.

4. The eclipses having the longest duration of totality on the central line did not take place until after November 8, 1817. On December 22, 1889, January 3, 1908, and January 14, 1926, totality lasted for 4.2 minutes at some points on the central line.

5. The last central eclipse will occur on July 18, 2232, and will be total but the duration of totality will be short.

6. The series changes from total to annular after July 18, 2232.

7. The series will entirely run out on November 4, 2412, with a very small partial eclipse. The moon will just graze the edge of the sun's disc.

WARWICK NECK, RHODE ISLAND, OCTOBER 21, 1943.

The Planets in December, 1943

By ALICE H. FARNSWORTH

NOTE: Greenwich Civil Time is used unless otherwise stated. To obtain Eastern Standard Time subtract 5 hours; Central Standard Time, 6 hours, etc. War Time in each zone is one hour later than Standard. Phenomena are described as seen from latitude 45° N. *The American Ephemeris and Nautical Almanac* is the source of the data.

Sun. The Sun remains south of the 20° parallel of south declination the entire month (α 16^h 24^m.6, δ — 21° 38′.0 on December 1; α 18^h 41^m.1, δ — 23° 6′.5 on January 1). Three days before Christmas at 17^h it reaches the winter solstice (δ — 23° 26′.7), then slowly moves northward. At this time when for northern dwellers its rays are so oblique and the duration of daylight so short, it is hiding from sight the rich treasure-trove in Scorpio and Sagittarius beyond which lies the center of our galactic system. On the map of the sky, it seems to be threading its way among old Novae, diffuse nebulae, and outlying globular clusters.

Moon. Lunar phenomena occur as follows:

		h	m
First Quarter	December	4	11 3
Full Moon		11	16 24
Last Quarter		19	20 3
New Moon		27	3 50
In perigee		1	10
Runs high		13	11
In apogee		17	7
Runs low		27	15
In perigee		29	2

In this calendar month, the Moon crosses the Sun's path three times, twice

at the descending node in Capricornus and once at the ascending node in Cancer. As a consequence, while south of the ecliptic, the Moon cuts across a corner of two constellations (Cetus on December 8, Orion on December 12) not included among the twelve zodiacal configurations.

The range in the Moon's declination has increased 2° during 1943. The present range (40°, from +20° to −20°) will further increase until 1950 when the attainment of its maximum value (57°, from +28°.5 to −28°.5) will indicate that the ascending node has proceeded westward from its present place in Cancer to the vicinity of the Vernal Equinox in Pisces.

Twice in this month the Moon passes through perigee. This brings to mind the numerous varieties of "month" which the badgered Moon provides. Leaving out of account the man-invented calendar months which may have values of 28, 29, 30, or 31 days, we have the following actual lunar periods (See Young's "General Astronomy" or *The American Ephemeris,* p. xviii):

Name	Average Length d h m s	Interval for passage from
Nodical	27 5 5 35.8	☊ to ☊
Tropical	27 7 43 4.7	♈ to ♈
Sidereal	27 7 43 11.5	* to same *
Anomalistic	27 13 18 33.1	Perigee to perigee
Synodic	29 12 44 2.8	New to New Moon

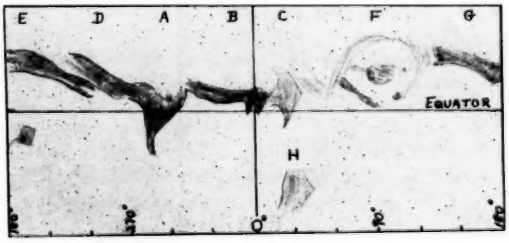
The close conjunction of the Moon with Jupiter on the morning of the 17th is visible as an occultation in Europe.

Mercury swings out to 20° east of the Sun on the 23rd, but will be difficult to see from northern latitudes where its path makes a small angle with the western horizon at sunset.

Venus is brilliant in the eastern morning sky, rising between 4 and 5 o'clock (War Time). On the 1st it is 4½° north of Spica.

Mars comes to opposition December 5, rising at sunset in the midst of Taurus, four times as bright as Saturn, farther east in the same constellation. The Moon passes the two planets on December 11.

At this opposition Mars is well north of the celestial equator, but offers a disk only 17″ of arc in diameter, two thirds its size as seen at favorable August oppositions. In a telescope of moderate size, with fair seeing, the observer will note the ruddy color of most of the surface, possibly a polar cap resembling



Sketch of a Few Prominent Features in Low Latitudes on Mars

a tiny white button, and in certain longitudes dark areas distinguishable from the red background in the center of the planet, but fading at the limb.

As a rule perhaps the most easily distinguishable feature is the triangular patch called Syrtis Major (A, λ 290° in the sketch) projecting downward into the northern hemisphere, flanked on the right by Sinus Sabaeus (B, λ 330°) in the equatorial region, with a double prong at its far end and the fainter Margaritifer Sinus (C, λ 25°) beyond. In the opposite direction from Syrtis Major you may strike the dark extent of Mare Tyrrhenum (D, λ 250°) and Mare Cimmerium (E, λ 200°), south of the equator. In the opposite hemisphere, likewise southerly in location is the mottled region of the Solis Lacus (F, λ 90°) with Mare Sirenum (G, λ 150°) following after. A marking well north of Margaritifier Sinus would be probably Mare Acidalium (H, λ 30°).

The Martian meridian of longitude 0° will be found central on the planet's disk at the times listed below. Each day it is central 37^m6 later than on the previous day. Longitudes which lie to the left on the chart will be central earlier than the indicated times by approximately an hour for each 15° difference in longitude; longitudes to the right, later. The last column shows the longitude central at 0^h G.C.T. on the specified dates. This quantity decreases about 9° per day.

1943	0° Longitude Central at G.C.T. ^{h} ^{m}	Meridian Central at 0^h G.C.T. °
Dec. 5	10 20.4	209
10	13 20.5	165
15	16 21.2	121
20	19 22.7	77
25	22 25.2	32
30	0 52.2	347

Of great interest are the results reported by E. C. Slipher[1] from visual observations and photographs made by him over a period of two months in South Africa, during the July opposition of 1939. From this southern latitude the planet, in Capricornus, could be viewed practically in the zenith. The season was spring on Mars and the southern snow-cap was seen to go through its usual decrease in size and change in aspect, normal for the season. Other portions of the surface, such as the region of Deucalion (south of Sinus Sabeaus) invisible in early spring, showed the wave of darkening observable with the approach of each Martian summer.

On the other hand, not all the changes observed on the planet were of a predictable nature. Over the north polar regions day-to-day photographs showed cloud areas, possibly formed of minute ice-particles, which lasted only a day or two. Remarkable and unexpected also were the changes, progressive in character, whereby a new dark marking appeared or familiar features grew notably less conspicuous. The first case is represented by a new marking in the bright region below Mare Cimmerium, invisible before 1924, showing weakly at that opposition and the following one, and very evident in 1939, "as if some dark coloration overspread the desert surface, in effect greatly reducing its reflectivity, and thus darkening it."[2] Definite reversals in the relative strength of certain canals leading away from the tip of Syrtis Major have occurred, an effect hard to explain.

[1] *The Telescope*, Sept.-Oct., 1940, p. 102.
[2] *Ibid.*, p. 107.

"These African photographs," writes Slipher, "record so many of the canals and oases in the positions, form, and character as depicted on Lowell's maps of the planet, that they should remove all doubt as to the reality of the markings."[3]

Jupiter, stationary on the 14th, remains about two degrees west of Regulus the entire month. The Moon occults Jupiter on the morning of the 17th, for observers in Europe, but passes north of Regulus. Observers are reminded again that mutual eclipses and occultations of the satellites may occur during these months. Inspection of diagrams for the month indicate that it might be worth while to watch the behavior of certain satellites on the dates given. W means that the pair will be found on the west side of the planet, E, on the east side. The phenomena would occur after G.C.T. $2^h\,45^m$.

Dec. 1	II and III	W	Dec. 20	Iand IV	E
3	II and IV	E	21	I and IV	W
12	I and IV	E	24	I and II	E
17	I and II	E	29	II and IV	E

Saturn, at opposition on the 16th, comes into good position for evening observation. It moves slightly westward from ζ Tauri during the month. The rings, nearly at maximum width, contribute fully at half-magnitude to the total brightness (-0^m3) of the planet.

Uranus, moving westward in Taurus, is favorably placed ($\alpha\ 4^h\,17^m$, $\delta +21°.3$ on December 15) for observation with opera glass or small telescope, in a group of stars of comparable brightness northwest of ϵ Tauri. Mars moves past it, nearly 3° north, on the 26th.

Neptune comes to western quadrature on the 26th. It moves from 1^m west of η Virginis to 1^m east of it during the month. The very close conjunction of the 8th-magnitude planet with the 4th-magnitude star occurs about December 10.5.

Charts showing the paths of the four planets Mars, Saturn, Uranus, and Neptune during December may be found in the January, 1943, issue of POPULAR ASTRONOMY (pp. 34-35).

[3] *Ibid.,* p. 109.

Occultation Predictions for December, 1943

(Taken from the *American Ephemeris*)

Date 1943	Star	Mag.	IMMERSION Greenwich C.T.	IMMERSION a	IMMERSION b	IMMERSION Angle E from N	EMERSION Greenwich C.T.	EMERSION a	EMERSION b	EMERSION Angle E from N
			h m	m	m	°	h m	m	m	°
OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′										
Dec. 6	54 B.Cet	6.3	2 45.6	—0.3	+2.3	11	3 29.0	—1.7	—2.8	294
9	B.D.+11°445	5.9	8 42.4	..	..	158	8 52.0	..	..	177
12	ν Gem	4.1	23 52.3	+1.0	+3.6	19	0 19.4	—1.0	—0.8	324
15	θ Cnc	5.6	10 45.8	—1.0	—1.7	109	11 58.2	—0.6	—1.8	285
23	13 Lib	5.8	11 49.8	—1.6	+0.4	104	13 7.4	—1.4	—0.8	310
24	190 B.Lib	6.4	9 22.1	—0.6	+2.1	72	10 8.3	0.0	—0.7	332
29	114 B.Cap	6.2	22 42.0	—0.7	—0.3	62	23 44.2	—0.6	—1.0	259
30	39 Aqr	6.2	21 19.4	—2.2	—1.0	108	22 13.7	—0.6	+1.3	203

Occultations Visible in Longitude +91° 0′, Latitude +40° 0′											
Dec. 3	182	B.Aqr	6.2	21 16.8	−2.0	−0.3	126	21 57.7	−1.0	+3.0	192
15	θ	Cnc	5.6	10 28.4	−1.3	−2.7	138	11 40.4	−1.9	−0.6	256
23	13	Lib	5.8	11 29.5	−0.6	−0.3	135	12 38.8	−1.5	+0.6	278
29	114	B.Cap	6.2	22 24.4	−1.1	+0.6	44	23 28.8	−1.5	−1.2	275
30	31	Cap	6.3	0 6.6	..	..	138	0 30.3	..	..	181
31	45	Aqr	6.1	0 44.6	−0.8	−0.1	59	1 48.8	−0.6	−0.7	253
Occultations Visible in Longitude +120° 0′, Latitude +36° 0′											
Dec. 9	B.D.+10°401		6.2	1 15.8	..	..	132	1 40.8	..	..	179
15	d^1	Cnc	5.9	4 35.5	−0.7	−1.3	149	5 7.3	+0.6	+3.5	213
30	ι	Cap	4.3	1 37.3	−0.7	+0.4	46	2 38.2	−1.0	−1.2	272
31	45	Aqr	6.1	0 11.1	−1.1	+1.8	26	1 12.9	−2.4	−1.2	282

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

Asteroid Notes

By HUGH S. RICE

The time given in these Notes, unless otherwise specified, is *universal time.*

Vesta is in a particularly fine position for observation for a considerable period this autumn and winter, opposition with the sun occurring December 5. During November and most of December this asteroid's motion is retrograde on a long thin loop, so it is retracing its August-October apparent path, being now less than ½° south of the former path, and the direction of travel on the celestial sphere due west. On December 10, Vesta crosses its path on August 21.

On November 12 at 0^h, Vesta's position appears to be coincident with NGC 1807, a star cluster at the Orion-Taurus boundary, and a "grupo bastante rico de estrellas . . ." according to Stuker, *Stern-Atlas,* the description also corresponding with that in the *New General Catalogue* of Dreyer. On November 19 Vesta is 0°.8 north of the star 11 Orionis. On December 4 it is 0°.3 north of Tauri. On December 13 it is 0°.4 north of Sigma-two Orionis. Soon after that it is in a very interesting position, for it passes by Aldebaran again, and will be nearer than in the August conjunction. On December 16 at 14^h Vesta is only 8′ south of Aldebaran. When observable in the U.S. on the night of the 16th, it is southwest of the star by a few minutes of arc. Its motion on succeeding days is directly west at 15′ of arc per day. On December 24 it is about 1/5° north of 75 Tauri, in the Hyades star cluster. There is no reason why observers cannot pick up the asteroid with a small telescope or even binoculars. The photomagnitude for December is predicted at 8; but the color of the planet is definitely pinkish or orange, as stated by German observers and confirmed by us, and the visual magnitude is probably 7.

27 Euterpe. While working in the region where Vesta lies, observers may wish to pick up Euterpe, which effects an apparent path parallel to and about

5° north of that of Vesta. Euterpe is probably a full magnitude or more fainter than Vesta, but still within range of small instruments. The opposition date is December 7.

On November 24 Euterpe is 2°.9 north of 104 Tauri and just south of a 6th-magnitude star; on November 29 it is practically coincident with another 6th-magnitude star; on December 11 it is 2°.6 north of 97 Tauri. On December 15 it is about 2° north of H.VIII 8, NGC 1647, described in the latter as a "cluster, very large, stars large, scattered" and in Webb as having stars of magnitude 8.5 to 10. The direction in the sky is westward until the middle of January, at which time it reaches the stationary point, 2½° north of Epsilon Tauri, at one end of the Hyades.

7 Iris. This asteroid is still to be seen in the evening sky in the west. Its photo-magnitude is about 10. It is now going direct, away from the star Beta Aquarii, and several stars of magnitude about 8 lie on its path. On November 28 it crosses its path of July 4. The ephemeris gives the planet's places.

Pallas. On October 17 this asteroid came to opposition, and so is still good for observation for a number of weeks. During November, December, and January, Pallas performs a small narrow retrograde loop, east and south of Beta Ceti, in a region with only faint stars. On January 8 the asteroid crosses its path of October 31. The positions can be given best by means of the ephemeris. The planet is bright enough for small telescopes.

Ceres. This huge asteroid is making another apparition (to employ an old word that is used especially with comets). It is at opposition December 24, and for a long time is very well placed for northern observers. The magnitude is about 8. On November 17 it is 1°.9 southwest of Epsilon Geminorum; on December 11, 3°.2 north of Mu Geminorum; but we give an ephemeris with which the observer may do his plotting, for Ceres' track lies among a myriad of 8th- and 9th-magnitude stars. It is a rich region, including many curious formations. On December 25 Ceres is 2½° north of the famous and magnificent star cluster M 35, NGC 2168, in Gemini. Continuing on its retrograde path, on January 10 it is immediately north of 136 Tauri.

The subjoined ephemerides are by courtesy of the Yale University Observatory.

ASTEROID EPHEMERIDES, 0^h U.T.

	7 IRIS			2 PALLAS			1 CERES		
		α	δ		α	δ		α	δ
1943		h m	° ′	1943	h m	° ′	1943	h m	° ′
Nov.	12	21 54.4	— 5 30	Nov. 12	1 8.2	—21 28	Nov. 12	6 36.0	+23 30
	17	21 58.6	— 5 13	17	1 5.9	—21 53	17	6 34.9	+23 49
	22	22 5.4	— 4 52	22	1 4.0	—22 10	22	6 33.1	+24 10
	27	22 12.7	— 4 27	27	1 2.8	—22 19	27	6 30.6	+24 32
Dec.	2	22 20.4	— 3 59	Dec. 2	1 2.2	—22 20	Dec. 2	6 27.3	+24 55
	7	22 28.6	— 3 27	7	1 2.2	—22 15	7	6 23.4	+25 19
	12	22 37.0	— 2 51	12	1 2.9	—22 4	12	6 19.0	+25 43
	17	22 45.8	— 2 12	17	1 4.2	—21 48	17	6 14.1	+26 6
	22	22 54.9	— 1 31	22	1 6.1	—21 27	22	6 9.0	+26 28
				27	1 8.5	—21 2	27	6 3.7	+26 49

Hayden Planetarium, American Museum of Natural History, New York, N.Y., October 25, 1943.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

The description of the August shower of Perseids is still being held up as it is not yet certain that all reports have arrived. It will probably appear in the December "Meteor Notes." Meantime some progress has been made in reductions of past observations both of fireballs and of routine reports from some of our best members. Among the fireballs, many can be solved but unfortunately others turn out to have such contradictory observations that all hope of solution is permanently abandoned. Here at Flower Observatory a better and more convenient system of cataloguing radiants determined from A.M.S. work is being worked up. We are not getting enough new active members, which is a great handicap, but one which can hardly be removed until the war is over. However, those who have recently joined as well as older members are urged to work when practicable so that the annual total of observations will not fall too far below the average.

North Carolina Fireball of 1936 May 4

On this night of May 4/5 at about 3 A.M., E.S.T., a great bolide crossed North Carolina and doubtless part of Virginia, ending not far from Salisbury, North Carolina, the general direction of motion being towards the southwest. Our most efficient regional director in that state, Dr. Milton L. Braun of Catawba College, Salisbury, undertook by requests through the newspapers and personal interviews to secure reports. Altogether he secured 15, but unfortunately all but one of those interviewed who could indicate the beginning and ending points in the sky, points which he then measured, were either in Salisbury, S2, or in Spencer, S3, which is only a couple of miles distant. This bunched the observers. A few other reports, except one at Elkin, S5, were too vague to be useful in deriving angles or distances. However, the Hydrographic Bulletin published the following: "Second Officer H. A. Feurtado of the American steamer Western Sword, . . . reports that on May 5, 1936, at 0759 G.M.T., in latitude 33° 01′ N, longitude 78° 32′ W, a brilliant meteor, about as large as Venus, appeared under Polaris at an altitude of approximately 31°. The body traveled slowly, leaving a trail which changed from bluish white to orange, and disappeared under Dubhe, at an altitude of about 10°, having been visible about 10 seconds. Weather . . . clear."

As the other observers reported the bolide at 0801 ± G.M.T., and the azimuths of the path as seen from ship coincide with the path, we would infer from the land reports, there seems no reasonable doubt that they all refer to the same object. It is true that from the ship, S1, the object was not so striking, but then it was very far off—entirely too far for the sound phenomena, for instance, to be detectable.

Attempting to solve the path in the usual way, I ran into difficulties which appear insurmountable. The main one is that, adopting the coordinates observed from S1, we get a beginning height of 407 km and an end height of 76 km *along* the azimuth lines given. But all the observations at S2 and S3 prove that the object passed both places and ended to the west, or even to the southwest. Using

the mean of 8 altitudes of the end point from these places, a height of 20 km is derived for a point corresponding to a projected distance of 42 km further southwest from the end as seen from S1. Again from Mooresville, North Carolina, S4, the positive statement is made that the object went from northeast to southwest, and we have 10 azimuths from S2 and S3, and altitudes for both the beginning point and one intermediate point on the path. These show an almost horizontal path, in fact such statements are made definitely by several observers, backing up their angular estimates. I am further puzzled by the fact that the points 407, 76, and 20 km all lie almost on a straight line which would give a slope of 43° for the path. But the 8 estimates for the beginning point as seen from S2, S3, and S5 give only 28 km, while the intermediate point, based on 5 estimates, is actually lower! To bring the altitudes of S1 into any agreement with the *known facts* that the bolide ended west of S2 and came from the northeast ±, we would have to assume that the altitudes from S1 were three or four times too great—an impossible error. So I am reduced to holding to only these certainties and regretfully admitting that the rest of the data seem wholly contradictory. The end point was over $\lambda = 81° 00'$ W, $\phi = 35° 40'$ N, within probably 5 miles in longitude and 15 miles in latitude. If erroneous in the latter, the point was further south not north. Its height was about 20 km.

The object was bluish in color and the trail left of short duration. At S2, the size was comparable to that of the full Moon, and the brilliance much greater. People within rooms with window shades down could clearly see objects so intense was the illumination. At least partial explosions or the breaking off of fragments seems to have occurred. Heavy rumblings and rattlings of windows were heard in Winston, Salem, Concord, and other places besides those mentioned. We know that the object was seen from both Virginia and South Carolina but no reports came directly to us from either state. The rumor that a meteorite fell in the Roaring River road near Elkin was promptly investigated by intelligent officials and the hole in the road found to have been made by a terrestrial accident. But for the unfortunate hour of its appearance doubtless this bolide would have been widely observed. As it was, many heard it, being awakened by the sounds, but few saw it. Dr. Braun put much time and trouble in collecting what data we do have, and, had all his letters asking for information been answered, the whole path might have been solved. Just two more reports from well-placed observers would have been sufficient. I am under many obligations to him for this and many other interesting cases whose full or partial solutions are due wholly to his efforts. It is a matter of keen regret that in most states we cannot find proper persons willing to act as regional directors. This fact is the greatest handicap to our work.

Fireball of 1938 February 7

At about 6:09 P.M., E.S.T., on this date, which was less than an hour after sunset for the whole region involved, a brilliant fireball appeared in the twilight, probably just north of the Virginia-North Carolina line and went southeast to end over a point west of Fremont in the latter state. We were particularly favored in that, out of a total of 12 reports from 4 states, nearly all gave data which were useful in one way or another in fixing the path. As usual for North Carolina fireballs, I am indebted for an observation first to Dr. M. L. Braun of Catawba College. He also put me in touch with Dr. K. H. Fussler, Department of Physics, University of North Carolina, who secured a valuable observation

from Dr. A. C. Howell and Mr. J. A. Southern of that University. Also I had four reports which he had collected in that state from my friend, Dr. E. C. Coker, Department of Physics, University of South Carolina. Others came here directly. In all enough are at hand to make a fairly good solution despite the handicap of twilight which prevented stars from being used as reference points. This was most fortunate as a long-enduring train, No. 1249 in F. O. Reprint No. 60, was left. When the latter reprint was published, I only had the direction of drift determined. In this paper other valuable data are derived. The list of stations and observers follows:

S1	Chapel Hill, N. C.	A. C. Howell and J. A. Southern
S2	near Columbia, S. C.	J. M. Sox
S3	Elizabeth City, N. C.	R. E. Lewis
S4	Salisbury, N. C.	M. L. Braun, Mrs. Braun, and others
S5	Hedgesville, W. Va.	through L. Hiett
S6	Columbia, S. C.	Mrs. F. J. Ford
S7	near Camden, S. C.	C. Williams and two
S8	Conway, S. C.	Mrs. C. J. Epps
S9	W. of Newport, Tenn.	J. F. Hawkins and A. T. Quartz
S10	Ruby, S. C.	J. K. Morrell
S11	Society Hill, S. C.	newspaper report
S12	Raleigh, N. C.	newspaper report

S3 had an unobstructed view of the whole path, gave all the angles with a diagram to check, and a most excellent and full observation. It is considered the key. S4 was inside and saw it from a window, the end well, but the beginning had to be checked by accounts of six others. This gave the observation a lower weight than that at S3. S1 did not see the fireball itself, only the flash; by the time they looked up the straight train was there, and they observed this. S5 was driving; his beginning azimuth disagrees somewhat with the three above which intersect almost in a point; his end azimuth agrees beautifully however. All the other azimuths are only rough, or give a general direction. Hence the beginning and end points depend upon the above. As to the heights, S1 did not see either end of the actual path, only its *direction* was observed, hence there are no altitudes given. The height of the beginning point depends upon the following S2, S4, S5, S8, and S10; and for the end S3, S4, and S10. S2, S5, and S11 all report that it reached their horizons, but we know that S2 states there was a pine wood near by, S5 was in hilly country, and probably the same reason applied to S11. The deviations from the mean are very bad for the beginning height; for the end height very good. I would have little faith in the former did not the weighted mean agree excellently with the value from S3, the key observation. The actual numbers and weights are: 146.4 (4), 71.2 (2), 240.0 (1), 210.6 (1), 146.9 (1), weighted mean 147.3 km; and 40.3 (4), 40.7 (4), 47.5 (2) weighted mean 41.9 km. The other data are found in the table.

Due to the strong twilight the long enduring train was seen only from S1, where it came within 10° of the zenith. Fortunately it was there plotted by two men who were most reliable and careful observers. Their diagram indicates a drift of the various masses of train-material, into which after one minute the originally straight train broke up, towards $a = 245°$. Using data in our table, I deduce that the center of the 10° to 15° train (assumed as 12° long therefore) was at a height of 87 km, the upper end at 92 km and the lower at 82 km, and its length 38 km. If the diagram is to scale, *i.e.*, the side motion proportional to the 12° length, then the wind-velocity was about 120 km/hour. Frankly, however,

I do not believe that the diagram was intended to be used in this way, so this figure while very plausible has little significance. As the train-height could not be foreseen, the good agreement with what has been derived from study of others, gives us added confidence in the data on the fireball's path.

The object was large enough to show a disc, which from S2 and S4 was estimated as being 1/10 to 1/5 that of the full Moon. Its brightness was greater than full moonlight in twilight. Its motion is reported as "slow," yet S1 and S3, which alone give estimates for duration, say "2 to 3" and "1 to 2" seconds. This would seem a great underestimate as the average value of 2 seconds would give an atmospheric velocity of 114 km/sec—one which I most positively think is far too great. I find no fireball about this date in the von Niessel-Hoffmeister Catalogue with a radiant near the one here given.

Date	1938 Feb. 7, 6:09 P.M., E.S.T.
Sidereal time at end point	45° 44′
Began over	$\lambda = 79°\ 40'$ W, $\phi = 36°\ 52'$; at 147 km
Ended over	$\lambda = 78°\ 10'$ W, $\phi = 35°\ 30'$; at 42 km
Length of path	229 km
Projected length of path	205
Observed velocity	114 km/sec (most uncertain!!)
Radiant uncorrected	$a = 139°$; $h = 27°.2$
Zenith correction (parabolic heliocentric velocity assumed)	$-3°.6$
Radiant corrected	$a = 139°$; $h = 23°.6$; $\alpha = 308°$, $\delta = +53°$

To the observers themselves and to those who aided me so effectively in securing the observations, I am under many obligations.

On 1936 Oct. 16 at about 4:50 P.M., C.S.T., a daylight fireball was reported from four stations in Indiana. The data at hand do not permit even an approximate solution of its path. It was visible about 5 seconds, was bluish-white in color, larger than "Venus at night" in size, had a short cone-shaped tail, but left no long-enduring train. It ended well west of $\lambda = 86°$ W, and probably south of $\phi = 41°$ N, coming from the east or northeast. As seen from Valparaiso, it was said to have exploded on disappearing. It would have attracted the widest attention had it appeared after nightfall.

Flower Observatory of the University of Pennsylvania, Upper Darby, Pennsylvania, 1943 October 6.

Contributions of the Society for Research on Meteorites

Edited by FREDERICK C. LEONARD
Department of Astronomy, University of California, Los Angeles

President of the Society: LINCOLN LA PAZ, Department of Mathematics, Ohio State University, Columbus

Secretary of the Society: C. H. CLEMINSHAW, Griffith Observatory, Los Angeles, and Department of Astronomy, University of Southern California, Los Angeles

References on the Early History of the Tucson, Arizona, Meteorites: The "Irwin-Ainsa" and "Carleton" Irons

By P. J. McGOUGH
Flagstaff, Arizona

ABSTRACT AND INTRODUCTION

The following 20 references, dating from 1850 to 1871, pertain to the early history of the famous Tucson, Pima County, Arizona, meteorites, the 688-kg., so-

called "Signet" or "Irwin-Ainsa Iron," and the 287-kg., so-called "Carleton Iron." A detailed account of these two meteorites, which concludes with an extensive bibliography on the subject, is contained in O. C. Farrington's "Catalogue of the Meteorites of North America, to January 1, 1909," *Mem. Natl. Acad. Sci.*, **13**, 460-7, 1915, to which the reader is referred for further information on them.

As the Gadsden Purchase was not authorized by the Congress of the United States until August 4, 1854, Tucson was a part of Mexico when the "Signet" meteorite was seen by John R. Bartlett of the United States and Mexico Boundary Commission, by J. L. LeConte, a physician connected with the Boundary Commission, and by Lieutenant John G. Parke, a United States Engineer.

The "Signet" or "Irwin-Ainsa Iron" is, according to Farrington ("Meteorites," pp. 74-5, 1915), the only known example of a ring-shaped meteorite. When first displayed at the Smithsonian Institution in Washington, D. C., it was the second largest meteorite found in this country. Its greatest external diameter is 49 inches, the central diameters ranging from 23 to 26½ inches; the main part of the mass, now on exhibition in Washington, weighs approximately 1400 pounds.

The "Carleton Iron" was in the possession of the Society of California Pioneers and was exhibited in the museum of that Society in San Francisco. After the earthquake and fire of 1906, the meteorite was removed to the museum of the California State Mining Bureau, also in San Francisco. In 1941, the iron was transferred to the Smithsonian Institution (the United States National Museum). It is 49 inches long, 18 inches wide, and from 2 to 6 inches thick; most of the specimen is, however, much thinner than 6 inches, so that the meteorite resembles a rusty plowshare. It weighs 632 pounds.

HISTORICAL REFERENCES ON THE TUCSON METEORITES

Ref. No. 1, 1850, Velasco

J. F. Velasco in *Noticias Estadisticas del Estado de Sonora, &c.*, p. 221, Mexico. The following is a translation from the Spanish:

"Between the presidio of Tucson and Tubac is a mountain range called Sierra de la Madera and a pass called Puerto de los Muchachos. In it are several enormous masses of native iron, and many have rolled to the foot of the said sierra. One of the masses, of a moderate size, was transported to Tucson and has stood for many years in the plaza of the said presidio."

Ref. No. 2, 1852, LeConte

Am. Jour. Sci., 2nd Ser., **13**, 289-90: "Notice of Meteoric Iron in the Mexican Province of Sonora," by J. L. LeConte, M.D. (Dr. LeConte was a member of the United States and Mexico Boundary Commission.)

"In February, 1851, while in Tucson in Sonora, I saw two large masses of iron, evidently meteoric, which were being used as anvils by the two blacksmiths of the town. They were irregular in form and, tho imbedded in the ground to make them steady for use, they were 3 feet high. I endeavored to have some pieces cut off, and, altho a high price was offered, their characteristic indolence could not be overcome; the only answer I could obtain was that the metal was *muy duro*. These pieces were brought from a valley in a small mountain chain about 40 miles southeast of Tucson, east of the road leading to Tuvaca; fragments similar to these and of various sizes were said to be abundant. The valley was called Cañada de Hierro, or Iron Valley, on this account."

REF. No. 3, 1854, BARTLETT

"Personal Narrative of Explorations and Incidents in Texas, New Mexico, California, Sonora, and Chihuahua, Connected with the United States and Mexico Boundary Commission during the Years 1850-53," **2**, 297-8, New York, by John Russell Bartlett, United States Commissioner during that period:

"July 18, 1852. In the afternoon I called to take leave of General Blanco and at the same time to examine a remarkable meteorite which is used as an anvil

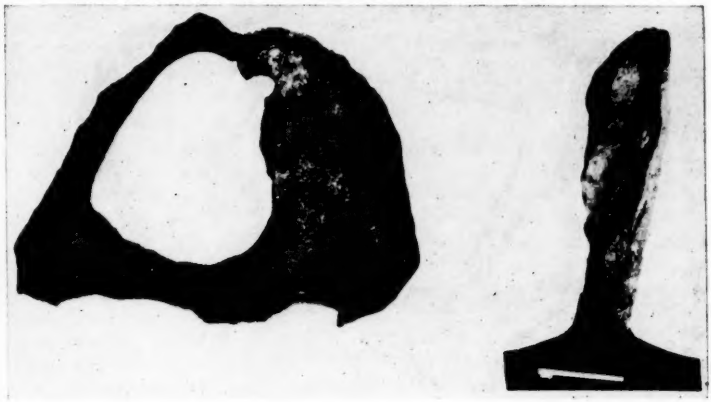


FIG. 1

THE TUCSON, ARIZONA, METEORITES:

The "Irwin-Ainsa" or "Signet Iron" (broadside view) and the "Carleton Iron" (as seen edgeways). (Photographs from the Chabot Observatory, Oakland, California. The "Carleton Iron" was photographed in San Francisco about 1936.)

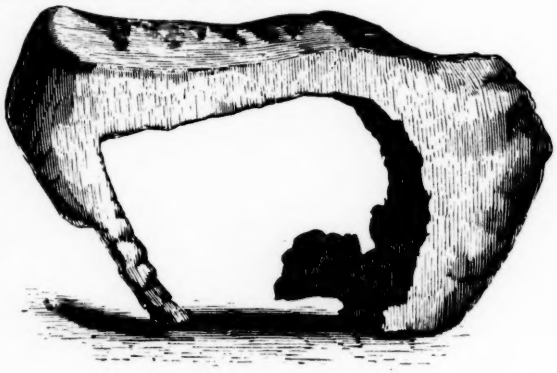
in the blacksmith's shop. This mass resembles native iron and weighs about 600 pounds. Its greatest length is 5 feet. Its exterior is quite smooth. It was found about 20 miles distant [from Tucson] toward Tubac. . . The annexed drawing [see Fig. 2] gives the appearance of the singular mass. There is another mass within the garrison grounds, of which I did not take a sketch. With much labor Dr. Webb broke off a fragment of this meteorite for the purpose of analysis."

REF. No. 4, 1854, PARKE

From "Reports of Explorations and Surveys to Ascertain the most Practicable and Economical Route for a Railroad from the Mississippi River to the Pacific Ocean, made by the Secretary of War, Jefferson Davis, in 1853-54, Lieutenant John G. Parke in Charge of the Survey from Doña Ana on the Rio Grande and the Pima Villages":

"February 31 [28?], 1854. Camp in Tucson. The Commandant showed me two specimens of a meteorite found in a canyon of the Santa Rita Mountains, about 25 miles south of Tucson. They are both used as anvils and were lying, one in the presidio and the other in front of the Alcalde's house. The one in the presidio is of a very peculiar form, being annular and somewhat like a signet ring of

large dimensions, its exterior diameter being about 3½ feet, the interior about 2 feet, and [it] weighs near 1200 pounds. By permission of the authorities, our blacksmith undertook to cut off some specimens for us, in which he almost failed, the metal being so tough and hard. By dint of two hours' hard work and [with] the use of a cold chisel of fine temper and a most weighty sledge, we procured a few small clippings sufficient for the purpose of analysis. The fracture is crystalline, resembling that of cast iron."



METEORITE AT TUCSON.—p. 298.

FIG. 2

THE "IRWIN-AINSA" OR "SIGNET IRON" AS IT APPEARED WHEN USED AT TUCSON AS AN ANVIL

This is a photograph of a page in Bartlett's "Personal Narrative," showing the larger Tucson meteorite as he sketched it in 1852. The piece of metal extending from the lower right-hand side is now gone. Bartlett, not knowing that the iron was ring-shaped, described it as standing on two legs.

REF. NO. 5, 1854, SHEPARD

"Notice of Three Ponderous Masses of Meteoric Iron at Tucson, Sonora," by Charles Upham Shepard, M.D., *Am. Jour. Sci.*, 2nd Ser., **18**, 369-72:

"The first intimation concerning the locality here noticed was afforded in 1851, at a meeting of the American Association for the Advancement of Science, on the occasion of my describing a meteorite from Deal, New Jersey, Dr. J. L. LeConte being present and having just returned from California through the Province of Sonora. In the month of February, previous, he observed two pieces of meteoric iron, which were being used as anvils by the blacksmiths of the town. He was unable to procure any specimens. Nothing further was brought to light until the present season, on the return of Lieutenant J. G. Parke, U. S. Topographical Engineer, from his scientific studies in Sonora, when he addressed me a letter of inquiry respecting them, attended with about an ounce for my examination, which he had procured in person. He remarked that the Commandant would not consent to removing the masses, even had he possessed the means, but, by dint of two hours' hard work, [he had] chipped off a few fragments, which I hope will serve the purpose of my analysis. The three masses were found in the Santa Rita Mountains, 25 or 30 miles south of Tucson. Two

were shown to him by the Commandant, both being used as anvils. To obtain these specimens would be attended with no little difficulty, owing to the remoteness of the locality. The route of transportation recommended by Lieutenant Parke is *via* Fort Yuma, 275 miles, thence by water to the head of the Gulf of California. Measures are already on foot for the removal of one of the masses to this part of the country, which it is greatly to be desired will be crowned with success. The masses were seen by officers of the Boundary Commission and are described in Bartlett's *Personal Narrative*."

REF. No. 6, 1855, SMITH

"Memoir on Meteorites.—A Description of Five New Meteoric Irons, with some Theoretical Considerations on the Origin of Meteorites, based on their Physical and Chemical Characters," by J. Lawrence Smith, *Am. Jour. Sci.*, 2nd Ser., **19**, 161-3:

"*Meteoric Iron from Tucson, Sonora, Mexico:* We have had several accounts of meteoric masses which exist at Tucson, Dr. J. L. LeConte having made them known some years ago. Since that time, Mr. Bartlett of the Boundary Commission has seen them and made a drawing which he has kindly allowed me the use of; his notice of them, however, is quite brief. The mass used as an anvil resembles native iron and weighs about 600 pounds. Its greatest length is 5 feet. Its exterior is quite smooth, while the lower part, which projects from the larger, is very jagged and rough. I have obtained fragments of the meteorite from Lieutenant J. G. Parke of the U. S. Topographical Engineers, who cut them from the mass at Tucson and to whose kindness I feel very much indebted. This mass is one of much interest, and it is to be hoped that some of our enterprising U. S. Engineers will yet be able to persuade the owners to part with it and bring it to this country."

REF. No. 7, 1855, GENTH

"Analysis of the Meteoric Iron from Tucson, Province of Sonora, Mexico," by F. A. Genth, *Am. Jour. Sci.*, 2nd Ser., **20**, 119-20:

"The masses of meteoric iron of Tucson were first brought to notice by Dr. J. L. LeConte and afterwards were more fully described by Professor C. A. Shepard and were mentioned also by J. L. Smith in this *Journal*, March, 1855, in which he gives the analysis of a piece cut from one of the larger masses by Lieutenant John G. Parke of the U. S. Topographical Engineers. Several months ago I had finished the following analysis of the same meteorite (which will be found to agree with those of Dr. Smith), but circumstances have heretofore prevented my presenting to the Academy the result of my examinations, which were made by pieces taken from the specimen presented to the collection of the Academy of Natural Science by Dr. Heermann."

REF. No. 8, 1855 AND 1857, MICHLER

"Report on the United States and Mexican Survey made under the Direction of the Secretary of the Interior, by William H. Emory, United States Commissioner and Major, First Cavalry; Franklin Pierce, President; Robert McClelland, Secretary of the Interior." The following is an extract from the report of Lieutenant N. Michler, Topographical Engineer, U. S. Army, who had charge of running the boundary line from the Colorado River eastward until it met the party running the line from El Paso westward:

"June, 1855. Tucson is inhabited by a few Mexican troops and their families,

together with some Apache Indians. Circumstances were such that my party and escort were compelled to remain encamped here for nearly the entire month of June. During this time, we became the recipients of every attention and civility from Captain Garcia, who commanded the place. A fine specimen of meteoric iron brought from the Santa Rica [Santa Rita] is to be seen at Tucson and is used as a blacksmith's anvil. It is massive and quite malleable."

See also N. Michler, "Report of the United States and Mexican Boundary Survey," **1**, Pt. 1, 118, 1857.

Ref. No. 9, 1863, Irwin

A letter to Professor Spencer F. Baird, Assistant Secretary, Smithsonian Institution, from Dr. B. J. D. Irwin, a United States Army surgeon who was stationed at Fort Buchanan, some miles from Tucson, in the year 1857. When he wrote the following letter, he was at the United States Military Hospital at Memphis, Tennessee. The letter was dated September 5, 1863, approximately six years after his discovery of, and claim to, the larger Tucson meteorite. (*Rept. Smithsonian Instn. for the Year 1863*, p. 65; see also Irwin and Ainsa, "On the Great Tucson Meteorite," *ibid.*, pp. 85-7.)

"My dear Sir: I am in receipt of your letter of the 25th ult., by which I learn that the great Tucson meteorite is on its way to Washington at last. I feel that you will feel proud of it when you see it. The only history I can give you is a vague one, as there is no written history of its advent in Tucson. The old inhabitants agree that it was brought in by the military stationed at the old presidio, where it remained until after the withdrawal of the Mexican garrison, it having been found in the Santa Catalina Mountains, which lie to the north of Tucson. It was taken from the garrison and set up on end where it was used as a sort of public anvil for the use of the inhabitants. In 1857, I found it lying in one of the side streets, partially covered in the earth, having been there evidently a considerable time. No person claiming it, I publicly announced that I would take possession of it in behalf of the Smithsonian and forward it whenever an opportunity offered. Mr. Ainsa agreed to have it taken to Guaymas for $50.00. The people of Tucson agree that a shower of meteorites fell in the Santa Catalina Mountains two hundred years ago. I wish that I could give you fuller information on this matter. Please let me know when you receive it and be assured that, when I go to Washington, I will pay my respects in person to you and it. Yours very respectfully, B. J. D. Irwin, Surgeon, U. S. Army."

Ref. No. 10, 1863, Ainsa

A letter to Professor Joseph Henry, Secretary, Smithsonian Institution, Washington, D. C., from Santiago Ainsa:

"San Francisco, California, July 2nd, 1863: The meteorite which remained so long at Alamito for want of a proper person to bring it here was brought by one of my brothers, Jesus M. Ainsa, who visited Sonora lately. We have been induced to retain it here for a short time, to satisfy the curiosity of the San Francisco people. The State Geological Society asked to be allowed to take a small piece for their collection, which request was granted. With this exception, the meteorite has been preserved entirely in the same condition in which it was found in Arizona, and, by the 13th of this month, we shall have the pleasure of shipping it to New York, under the care of the Pacific Steamship Company. I

take this opportunity to offer my services to the Institution. I remain Respectfully yours, SANTIAGO AINSA."

REF. No. 11, 1863, AINSA

A letter to Professor Joseph Henry, Secretary, Smithsonian Institution, Washington, D. C., from Santiago de Ainsa:

"San Francisco, California, August 26, 1863: I have the pleasure to acknowledge your favor of July 31st and take pleasure in complying with your request; in fact, I intended to do this before, but, owing to many engagements, I have postponed it to this moment. I announced in my last letter that the meteorite would be sent by the following steamer of that date, but we were asked to retain it, by some scientific men, who wanted to examine it closely. The history we have of the meteorite we have from our grandmother, Doña Ana de Islas, daughter of Don Juan Bautista de Anza, our great-grandfather. The Jesuit missionaries had the earliest knowledge of this meteorite. There were various theories entertained about it, but it was generally believed to have proceeded from some iron mine in the vicinity. In an expedition by Don Juan Bautista de Anza, then *Gran Capitán de las Provincias del Oriente,* about the year 1735, to the country about Tucson, he was induced to visit the meteorite and he undertook to transport it to Spain. The place where it was found is called Sierra del Madre. Through the want of proper means and the bad state of the roads, having to carry it to San Blas, then the nearest port of entry, the work of transportation was given up, and they were satisfied to take it as far as Tucson. There it has remained ever since, until my brother, Augustin Ainsa, undertook to transport it to Guaymas, in 1860, and to present it to the Institution. His intentions, however, were not carried out until May last, when another brother, Jesus M. Ainsa, visited Sonora and brought it with him on his return. By the time of the receipt of this, the meteorite must be already in Washington, as we delivered it to the agent of your Institution about a month ago, to have it transported to you. Your agent spoke to us about the expenses, but we wish not to deprive ourselves of the honor of having presented it to the Institution, inasmuch as we desire that you will accept it. I have the honor to remain Your obedient servant, SANTIAGO DE AINSA."

REF. No. 12, 1863, CARLETON

A letter addressed to the Board of Supervisors of the City of San Francisco, from General James H. Carleton, [received?] November 24, 1862, and read by Professor J. D. Whitney, State Geologist, at the meeting of the California Academy of Sciences, May 4, 1863 (from *Proc. Calif. Acad. Nat. Sci.,* **3**, 23; see also two papers by Whitney, "On Meteoric Iron from Arizona," and "Remarks on the Nature and Distribution of the Meteorites which have, up to the Present Time, been Discovered on the Pacific Coast and in Mexico," *ibid.,* pp. 34-5 and 49, and 240-1, respectively):

"Headquarters, Column from California, Tucson, Arizona, January 30, 1862: To General George Wright, U. S. Army, Commander, Department of the Pacific, San Francisco, California: My dear General: Soon after my arrival at this place, I sent by train to Fort Yuma, to be shipped to you, a very large aerolite [siderite], which I found here and which I had read [about] and heard of for many years. In Bartlett's *Personal Narrative,* it is described as follows. . . [Here follows an excerpt from the forenamed book.] . . . I desire that you present this to the City of San Francisco, to be placed upon the plaza, there to remain for

the inspection of the people and for examination by the youth of the city forever. It will be a durable memento of the march of the Column from California. Sincerely, JAMES H. CARLETON, Brigadier General, U. S. Army."

Bartlett did not see the aforementioned meteorite; *he* described the *larger* of the two Tucson meteorites, namely, the "Irwin-Ainsa" or "Signet Iron." The meteorite discussed in the preceding letter—the smaller of the Tucson specimens—is known as the "Carleton Iron." It was placed originally in the museum of the Society of California Pioneers, in San Francisco. After the earthquake and fire of 1906, it was removed for safe keeping to the museum of the California State Mining Bureau, likewise in San Francisco. Finally, late in 1941, it was transferred to the Smithsonian Institution (the United States National Museum), in Washington, D. C., there to rejoin its companion, the "Signet" or "Irwin-Ainsa Iron." Hence, both of the Tucson meteorites are now on exhibition in the National Museum.

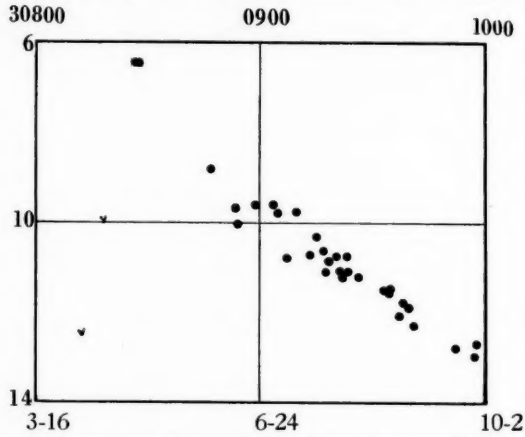
[*To be continued*]

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Nova Aquilae, 1943: Word has been received recently of the discovery on September 5 by Hoffmeister at Sonneberg, Germany, of a twelfth magnitude nova in the constellation of Aquila. It was also announced that the apparent outburst



PHOTOGRAPHIC LIGHT CURVE—NOVA AQUILAE, 1943

occurred between April 13 and May 2 of this year when the star attained the seventh magnitude. The Harvard plates, when examined by Dr. S. Gaposchkin, revealed a considerable amount of observational data, as shown in the accompanying figure which carries the observations up to September 28. These observations

narrow the time of the outburst to the interval between April 15 and 28 and also show that the nova reached at least the magnitude 6.4 (phtg) on May 1, 1943. Accordingly, it possibly may have attained naked-eye visibility at some time. This is the tenth nova which has been found in Aquila, a number exceeded only by those found in Sagittarius, where twenty-two have been detected.

Variable Star Observing in England: From the report of the Director of the Variable Star Section of the B.A.A., we learn that variable star observing is being rather actively carried on in England. The B.A.A. observers communicate their results only annually, instead of monthly, so the actual number of observations made is not known until after the end of the current year. The report ends with some very appropriate hints to observers which are well worth reprinting:

Be sure that the field, the variable, and the comparison stars are correctly identified.

Try to adjust your head so that the line joining the variable and the comparison star is parallel to that joining your eyes. Avoid an estimation in which the lines are at right angles to each other.

Do not attempt to observe faint stars until your eye is thoroughly dark-adapted.

Use averted vision only when direct vision will not serve.

Observe with the star as near center of field as possible. Use lowest power, largest field eyepiece as possible.

Use at least two comparison stars every time, one brighter, the other fainter than the variable.

Each observation should be made, as far as humanly possible, unbiased by previous observations, or by expected behavior of the variable.

The A. A. V. S. O. Meeting: The thirty-second annual meeting of the A. A. V. S. O. was held at Harvard Observatory on October 8 and 9. That the holding of such a meeting was justified is evidenced by the fact that some sixty members and guests were in attendance at the different sessions. To be sure, they came only from New York and New England, with a dozen from the state of Maine itself.

The Bond Astronomical Club was, as has been the custom for several years past, host to the Association on Friday evening. Dr. B. W. Sitterly, now on leave from his regular duties at Wesleyan College, Middletown, Connecticut, was the speaker of the evening. His topic, "The Finding of Stellar Distances," though perforce a technical one, was clearly expounded and proved to be of great interest to the more than a hundred listeners present. Following the lecture, members of the two organizations had the opportunity to renew old acquaintances and to exchange ideas.

The Council of the A.A.V.S.O. met in the lounge on Friday afternoon. Three active members were duly elected to life membership: Dr. E. Dorrit Hoffleit, on leave from Harvard and now for the duration stationed at Aberdeen, Maryland; Mrs. Florence Gooch Woods of Woodsville, New Hampshire; and Mr. Edwin E. Friton of St. Louis, Missouri. Eight persons were elected to active membership:

Mrs. Ada M. Arrowsmith, St. Albans, New York.
Miss Jessie L. Beach, Portland, Maine.
Mr. J. Edgar Guimont, Montreal, Canada.
Mr. Herbert J. Hopkins, Old Orchard Beach, Maine.
Mr. James C. Robinson, Clovic, New Mexico.
Mr. Alan R. Sandage, Oxford, Ohio.

Mr. Russell T. Wolfram, Mt. Hamilton, California.
Mr. Laurence D. Yont, Concord, Massachusetts.

The Treasurer reported a healthy condition of the treasury, mainly because of the fact that many members, long in arrears, had paid up their back dues. Perhaps the generally prosperous condition of the country is thus reflected. The Council also voted to award the seventh merit award, the highest distinction which can be voted to a member, to the retiring past-president, Charles W. Elmer of Glenbrook, Connecticut, and Southold, Long Island, New York. For more than twenty years Mr. Elmer has been closely identified with every main activity of the Association and such recognition is justly deserved.

The regular business meeting was called to order at 10:00 o'clock on Saturday morning, President Brouwer in the chair. Following the reports of the Secretary and Treasurer, respectively, tributes were paid to four deceased members; to Dr. Frank Schlesinger, by President Brouwer; to Professor James R. Jewett, by Dr. Shapley; to George R. Griffin, by Mr. Harris; and to George E. Ensor, by Mr. Campbell.

The annual election of council members for two years resulted in the choice of Miss E. Dorrit Hoffleit, Miss Marjorie Williams, Dr. Wm. L. Holt, and Mr. Lewis J. Boss. At the session of the newly formed council the following were elected as officers for the coming year: President, Roy A. Seely; 1st Vice President, Charles H. Smiley; 2nd Vice President, Margaret Harwood; Secretary, David W. Rosebrugh; Treasurer, Percy W. Witherell; Recorder, Leon Campbell. The last three were re-elections to those respective offices!

Probably the most active committee is that which handles the occultation work, as reported on by Miss Farnsworth, chairman, and Dr. Brouwer, who is preparing the data for publication. The 45 occultations observed in 1942 have been reduced. Of the 80 observed in 1943, 45 have been reduced.

Interest in sun-spot activity was sufficient to suggest that, now that it is difficult, or even impossible, to communicate such observations to Berne, they be sent to Dr. Shapley at Harvard who will see that they are duly forwarded to a suitable center in this country, until after the war at least.

Summation of the year's work on the part of the Association as a whole is usually contained in the report of the recorder. The outstanding feature was, undoubtedly, the visual discovery of Nova Puppis last November by its own member, Professor Bernhard H. Dawson of La Plata, Argentina. For this achievement, Dr. Dawson was awarded the David B. Pickering Nova Medal of the Association, the first member to receive this award for a visual discovery of a nova. The medal was formaly presented to the recipient on September 11 by the Chargé d'affaires of the United States Embassy at Buenos Aires in conjunction with the dedication ceremony of the new observatory of the Asociación Argentina Amigos de la Astronómia, of which Dr. Dawson was president for many years.

Besides the 500 observations of Nova Puppis as made by the A.A.V.S.O., an equal number came to hand from other sources, so that now the light curve is completely observed from the day of its discovery into July of this year.

The library and slide collection of the Association were little used outside of Cambridge, but acquisitions to the library numbered 176.

Three numbers of Variable Comments were issued, besides the Bi-Monthly Bulletins and the reprint of Variable Star Notes in POPULAR ASTRONOMY for 1942. Publication of the observations themselves is practically complete through 1941, and copy for printer is finished through 1942.

The discussion of the light curves of 400 long-period variables, involving the use of over 600,000 observations made between the years 1922 and 1942, has been completed and the results should appear in print in the Annals during the next year. As a by-product of this task, there is also available the derivation of approximately 20,000 dates of maxima and minima of these same stars for the same years.

During the year ending September 30, there were 31,205 observations contributed to headquarters by 80 observers. Although this total is slightly under that for the previous years, yet the continuity of the current light curves seems not to have suffered materially. The far southern variables appear to have suffered the most, with so few observers now able to make observations of these stars.

Again heading the list of contributors is Cyrus F. Fernald of Wilton, Maine, who made 4536 estimates on more than three hundred variables. He observes with an 8-inch Springfield-type reflector and probably holds the record for the number of observations per hour, and per night, through the year. R. P. de Kock of Cape Town was second on the list with 3054 observations to his credit. Were it not for the efforts of de Kock and Cousins, both of South Africa, the far southern stars would be very sadly neglected.

Foreign observers contributed 9698 estimates; 4248 from the four in South Africa; 2463 from seven observers in Canada; 1232 from three in Mexico; and 685 from three South American observers. The two observers in India communicated 630 estimates, valuable even if belated, and the two in Australia sent in 428 observations. The 59 observers in the United States contributed 21,519 estimates.

Outstanding as an observer was Rev. T. C. H. Bouton of St. Petersburg, Florida, now in his 87th year, and for 32 years—ever since the work of the A.A.V.S.O. started—an active observer. Also mention was made of the splendid record of Mrs. Winifred C. Kearons of Fall River, Massachusetts, who made 1528 estimates, and ranked fifth in the list. As has happened on so many occasions, the ten highest ranking observers made more than sixty per cent of the total number of observations contributed.

At the close of the 32nd year of continuous activity on the part of the A.A.V.S.O., more than 900,000 observations of variable stars have been made. These have appeared in print, first, and for many years, in the pages of POPULAR ASTRONOMY, and later in the *Harvard Annals*.

At the afternoon session Messrs. Rosebrugh and Seely discussed their methods of determining times of minima of Algol. Mr. Rosebrugh also described his own method of observing Nova Puppis in the gradually darkening twilight sky. The recorder demonstrated light curves of the four well-known and well-observed R Coronae Borealis stars, pointing especially to the fact that all four of these stars underwent below-maximum variation late in 1942, and that three of the four displayed a similar feature in 1938. Light curves of Z Andromedae and Gamma Cassiopeiae, as well as of the newest novae in Puppis and Aquila were also shown and described.

Dr. Brouwer spoke at some length on the progress made during the last few years in increasing accuracy and avoiding systematic errors in the determination of stellar positions from photographic plates.

Following adjournment, all repaired to the Residence to partake of tea and

cookies, and indulge in a chance to chat. This meeting may perhaps not be described as so scientific as have been previous meetings, but there was certainly much more opportunity for members to get better acquainted and to discuss more intimately their different problems and their own methods of observing.

At the dinner held Saturday evening, at which fifty-one were present, Dr. Dean B. McLaughlin recounted the history of the American Astronomical Society, of which he has been secretary for several years. Dr. Bart J. Bok told of the work of his committee in the distribution of astronomical publications to and from foreign countries. Dr. Shapley reported on the low and high lights in astronomy during the past year. The main low-light concerned the Diamaca-Peltier comet which evidently moved so strangely and raipdly across the northern sky that accurate knowlege of its whereabouts is lacking, at least as far as American astronomers are concerned. Among the high-lights were the already mentioned discovery of Nova Puppis and the nova in Aquila found in September by Dr. Hoffmeister. Probably the greatest contribution to astronomy, and science in general, has been the many new and novel gadgets which have been in the making behind closed doors, and which, when they come to light after the war, will greatly enhance our methods of observation.

Observers for September, and Annual Summary: Herewith is given the list of observers for September, with the number of variables observed and estimates made, and, in the last column, the total number of observations made by each observer for the year.

	Var.	Est.			Var.	Est.	
Ashbrook			5	Houston			25
Ball			24	Howarth	10	10	197
Bappu			438	Hudlow			5
Blunck	19	30	296	Huffer			10
Boone			108	Jones	54	120	599
Bouton	26	35	729	Kearons	74	134	1528
Buckstaff	23	79	395	Kelly	12	19	173
Campbell			17	de Kock			3054
Carpenter			8	Koons			821
Chandra			192	Krumm			185
Cousins	59	153	1004	Labrecque	23	48	163
Dafter			231	Lange			4
Dawson			16	Leavitt			6
Duffie			230	Lovinus			26
Duncan			2	Luft			349
Erdman			76	Manlin	63	63	354
Escalante			324	Mason			3
Fernald	273	624	4536	Maupomé, A. S.			833
Ford	19	19	68	Maupomé, E.			75
Friton			21	Mayall			42
Garneau	39	56	305	Meek	40	260	1352
Griffin			72	Moore			26
Halbach			107	Nadeau	116	177	1152
Hale			15	Oravic	15	44	44
Harris, R.			8	Palo			47
Harris, H. M.			373	Paraskevopoulos			5
Hart			35	Parker, E.			6
Hartmann	76	85	2353	Parker, P. O.			317
Heckencamp			33	Parks	26	48	444
Herbig			134	Peltier	165	254	1990
Hiett	22	34	201	Pouliot			11
Hinsham			7	Reeves	3	8	26
Holt	99	131	1298	Robinson			27

	Var.	Est.			Var.	Est.	
Rosebrugh	7	85	722	Topham			272
Sandage	45	73	183	Treadwell			5
Sanders			22	Vohman	25	37	329
Schoenke	37	48	330	Webb	15	19	186
Segers	9	19	324	Weber	120	120	761
Sill	43	45	506	White			1
Stahr	3	5	5	—			
Stone	3	3	3	80 (Totals)		2885	31,205

October 15, 1943.

Comet Notes

By G. VAN BIESBROECK

COMET DIAMACA-PELTIER. This new object was discovered at the national observatory of Bucharest (Rumania) on September 10. The following information was made available by that institution.

1943 September 10 at $2^h 24^m.9$ Universal Time
Right Ascension $8^h 35^m.4$ Declination $+53° 1'$
Magnitude 8—Daily motion $+12^m 43^s$ and $+2° 25'$.

At Bucharest a program of photographic search for asteroids is carried on and the new object has probably been encountered in the course of that work.

The normal channels of telegraphic communication are sadly hampered at this time. In Europe the Central Bureau located at Copenhagen cannot correspond with this country. The information had to come by way of Zurich (Switzerland) from where it was sent by radio to the Harvard College Observatory, central station of information in this country. The data were then relayed to American observatories, but all this caused such delays that the information did not become available until September 17. By that time the rapid daily motion of more than three degrees had carried the comet far from the discovery position in Ursa Major. Nobody seems to have picked it up until it was independently found by the veteran comet hunter L. C. Peltier at Delphos (Ohio). He gave the following rough indication:

1943 Sept. 18 $15^h 0^m +59°$ Magnitude 10
19 $15^h 40^m +54°$

which now located the object in Draco. This showed that the comet had moved even faster than the dicoverer indicated and probably passed its closest approach to the earth.

No further information has become available about this run-away object. Most astronomers are away from their telescopes under the present circumstances and only further accidental recovery would make it possible to establish something about the orbit and future course of this comet. There remains a possibility that observations might have been obtained in Europe where the discovery was sooner made available but it will be some time before anything is learned about that here.

Except for the faint comet SCHWASSMANN-WACHMANN No. 1 for which an ephemeris was given in the August issue (p. 405) no known comets are under observation now and nothing has been learned about the expected periodic ones that were mentioned last month.

McDonald Observatory, Fort Davis (Texas), October 7, 1943.

General Notes

Frank Schlesinger Memorial Exercises.—Memorial exercises for Professor Frank Schlesinger who died on July 10, 1943, will be held in Strathcona Hall of Yale University at 4:30 P.M. on Friday, November 19, 1943. The exercises will be presided over by the President of the University, Dr. Charles Seymour, and the speakers will be President-Emeritus James Rowland Angell and Professor Henry Norris Russell of Princeton University.

Dr. Felix Aguilar.—The director of the Astronomical Observatory of the University of La Plata, Dr. Felix Aguilar, died suddenly on September 28, 1943. Mr. Aguilar was not only one of the leading astronomers of the Republic of Argentina, but he was also active as president of the Comisión para la Medición de un arco de meridiano en la República Argentina, president of the Comisión Nacional de Observatorios, and member of the Comisión de límites Argentino-Chilena. He served as professor of geodesy in the University of La Plata and was a member of several scientific associations such as the Sociedad Cientifica Argentina and the Academia Nacional de Ciencias. At the La Plata Observatory his name was connected with the creation of the Escuela Superior de Ciencias Astronómicas y Conexas and with the Instituto Geográfico Militar where he served as head of the geodesy department. He was active in the determination of the differences in longitude between Potsdam and Belgrano (Buenos Aires), and participated in the first gravimetrical and magnetic investigations of the Instituto Geográfico. In 1936 he was placed in charge of the reorganization of the Observatorio Astronómico Nacional at Córdoba. He was the author of many scientific papers and invented a method for the use of calculating machines in the determination of latitude by a method of Gauss. He undertook one of the few astronomical leveling operations that have ever been executed. A short time before his death he arranged for two of his associates, Dr. Carlos Cesco and Dr. Jorge Sahade to come to the United States for research in astrophysics.

The Committee for the Distribution of Astronomical Literature, (C.D.A.L.) has issued Bulletins Nos. 11-16, inclusive, each pertaining to the recent astronomical publications in one of the countries in Europe with which regular communications are interrupted at present. In order, the Bulletins relate to France, Holland, Sweden, Germany, U.S.S.R., and Italy. The information from the enemy countries has been made available usually through one of the few remaining neutral countries. It is a tribute to the persistence of certain of the scientific workers that so much work is being done in such troubled times. These Bulletins are in addition to the regular Monthly Astronomical Newsletter issued by the same committee.

Book Reviews

Norton's Star Atlas, Ninth Edition. (Messrs. Gall and Inglis, 13 Henrietta street, London, and 12 Newington Road, Edinburgh.)

This work first published in 1910 has now reached its ninth edition. The

continued demand for this work, as indicated by the successive editions, is conclusive evidence of its usefulness. Reviews were given in this magazine of the sixth and of the seventh editions, the latter in Volume 48 (1940), page 228. These reviews were very favorable and commendatory. The same good impression is created by an examination of the copy of the ninth edition now before us.

Navigation, by J. C. Kingsland and D. W. Seager. (Oxford University Press, 114 Fifth Avenue, New York. $1.00.)

A knowledge of navigation is essential to flying; yet the study of navigation is so comprehensive and involved that its full treatment would fill many volumes. This little book is designed to give a background of the art of navigation for the use of flyers whose training is obviously too short to allow a thorough study of the subject. In spite of the issue of new maps, the development of new instruments, and the changes made in methods, the broad principles of navigation remain unchanged. "Navigation" will, therefore, be of inestimable help to the student navigator in acquainting him with the fundamental principles of the art. There are chapters on The Form of the Earth; Maps and Charts; Magnetism and the Compass; Meteorology, which includes a discussion of the altimeter and air-speed indicator; and Dead Reckoning Navigation. Practical exercises following most of the chapters, with their solutions given at the end of the book, make "Navigation" the ideal handbook for all who are in any way concerned with flying. (*From the Publisher's notice.*)

Learning to Navigate, by P. V. H. Weems, Lt. Comdr. U.S.N. Retired, and William C. Eberle, Lt. Comdr. U.S.N.R. (1943 edition, 135 pages. Pitman. $2.00.)

Nearly every one who has taught or practiced navigation in the last two decades knows Commander Weews and/or his books and aids for the navigator.

This small, adequately illustrated and very readable book is a good introduction to the science and art of navigation, in both of which the authors are masters. It is also the kind of a book which the experienced navigator picks up and reads with real pleasure.

The first eleven chapters cover all the essentials of navigation at sea. The last chapter on Air Navigation is quite concentrated. Tables and samples of larger tables are included. Bibliography and Sources of Information near the end of the book are sufficiently inclusive.

The former seems to be quite heavily loaded with the name of Weems, himself. This is perhaps a matter of business rather than taste.

The standard works by the U.S. Naval Observatory and Hydrographic Office as well as the Civil Aeronautics Bulletins are listed.

The book contains practically no mathematical equations. This alone should make it a best seller with an interested public at the present time. Skill, in bypassing the formalities of mathematics above, below and beyond, by means of word reasoning, ingeniously devised tables and graphical methods, has developed greatly in recent years and notably so in navigation.

This small volume is worthy, in this reviewer's opinion, of being made the nucleus of a small library on the subject whether the collector be an amateur or a professional navigator.

OLIVER J. LEE.

Solar Relations to Weather, by H. H. Clayton, 2 volumes. Volume 1, 105 pages; Volume 2, xiv + 439 pages. (Clayton Weather Service, Canton, Massachusetts. $3.00.)

At this time when problems of the weather are both of an immediate concern and growing importance, Mr. Clayton's 2 Volumes, "Solar Relations to Weather" make conveniently available the results of a noted Meteorologist's contributions of a lifetime spent in investigations that have an important bearing on the art and science of forecasting.

Volume I is an up-to-date summary of the high points of Mr. Clayton's findings based on years of investigations of the distributions of barometric pressure and temperature changes, with variations in solar radiation and the sunspot cycle.

Volume II is a comprehensive collection of the author's technical papers reproduced from various scientific journals. This collection puts together in one volume material which would have to be located in such widely distributed publications as *The American Meteorological Journal, The American Journal of Science, Smithsonian Publications, Bulletin of the American Meteorological Society, The Journal of Terrestrial Magnetism and Atmospheric Electricity, Popular Astronomy,* and *Science.*

Mr. Clayton, during his long career, has been connected with the Blue Hill Observatory, the United States Weather Bureau, The Smithsonian Institution, and for many years was Forecaster in charge of the Forecast Division of the Argentine Weather Bureau. These Volumes give the results of over 60 years of research in world-wide weather conditions and their possible relations to solar changes.

One who has carefully read these Volumes will recognize the force of accumulating evidence that the major zones of high and low pressure in the Earth's atmosphere which are fundamentally responsible for all our weather, are definitely related to the solar cycle as marked by sunspots. Through a method of analysis similar to that which reveals the annual change in the distribution of pressure with the season, Mr. Clayton traces the changes in pressure over the Earth's surface that accompany solar changes following the sunspot cycle. Whether solar activity is measured by sunspot numbers, faculae, or flocculi, it seems one cannot escape the conclusion that at sunspot maxima the Earth's atmospheric pressure is below average between latitude 40° north and latitude 40° south of the equator, while at latitudes above 40° the pressure averages higher at sunspot maxima than at sunspot minima. These differences appear to be intensified with increased solar activity so that the actual locations of these pressure zones seem to depend upon the degree of solar activity attained at a given sunspot maximum.

The perusal of these Volumes will result in a firmer conviction as to relationships between the Sun and the weather than has been generally held. These Volumes should form a starting point for further progress in a method for longer-range weather prognostication than has yet been attempted on anything like an adequate scientific basis.

HARLAN T. STETSON.